PROCEEDINGS

OF

ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

PITTSBURG, PA.

VOL. III.

1885, 1886, 1887.

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Engineers' Society of Western Pennsylvania.

Record of Annual Meeting, January 20th, 1885.

A. DEMSTER moved that Mr GOTTLIEB take the Chair. Motion seconded, and carried.

Mr. GOTTLIEB called the meeting to order, and the Secretary read the minutes of the previous meeting.

A. E. FROST, Treasurer, read his Annual Report, which showed receipts amounting to $1,405.71; expenditures, $1,365.11, aving a balance on hand of $40.60.

JAS. H. HARLOW, Secretary, read his report, showing an increase in membership of 30, making a total membership of 85—accessions, 47; loss by resignation, 15—by death, 2.

WM. SCHERZER, Librarian, read his report, which consisted of a detailed statement of the periodicals subscribed to by the ociety, 18 in all; those received in exchange for its publications, 19 in all; and bound publications presented to the Society, 28 all.

The Committee on Tests was called upon to report, but made no report. The address of the retiring President was then ead by the Secretary, JAS. H. HARLOW. In this address the President referred to the joint meeting of our Society with the merican Society of Mechanical Engineers,—urged the members to attend the meetings more promptly, and especially the ounger members, as they will thus best promote the objects for which the Society was organized, and to look upon the payment their obligations to the Society regularly and at the beginning of the year. The address refers to the unfortunate loss the ociety sustained in consequence of the failure of the Penn Bank, and calls attention to the handsome addition to the library, onated by WM. METCALF, Esq., consisting of twenty-six bound volumes of "Engineering." Before closing this address brings efore the Society the advantages that can be derived from a series of lectures on Chemistry, Geology and other scientific subcts, delivered by men of acknowledged ability in the domain of Engineering.

ELECTION OF OFFICERS.

The Chair appointed Messrs. CHAS. DAVIS and A. DEMSTER, Tellers. The Chair announced the election as follows:

GEO. H. BROWN, President.
C. L. STROBEL, Vice President.
WM. MARTIN, } Directors.
WM. THAW, JR., }
A. E. FROST, Treasurer.
WM. F. ZIMMERMANN, Secretary.

Recess of ten minutes was taken.

Meeting opened with C. L. STROBEL in the Chair.

The Board presented the names of G. B. MITCHELL, CHAS. M. CLARKE. CHAS. REISINGER, CHAS. BECKFIELD, F. K. ILTER and JAMES MALLEN, for ballot.

On motion, the Secretary was directed to cast a ballot admitting them to membership.

The questions asked by Senator MORGAN about Natural Gas were read by the Chair, and were responded to by Messrs. OBERTS and JARBOE, and Capt. JONES. These gentlemen's remarks showed that steam would prevent the deposit of coke furnaces only to some extent, and that natural gas has been known to flow from wells that had carried oil and gas after the il has ceased to flow.

JAS. H. HARLOW said he was requested to read the following resolution:

Whereas, The recent accident to our esteemed President, WM. MILLER, by which we were threatened with a sad loss, brings orcibly to our notice an evil which by long usage we have come to passively tolerate, but which should never have been pernitted to be originated, much less continued, and should at once be abated; therefore, be it

Resolved, That the use of iron for cellar doors, vault coverings or gratings, when they form part of the pavement used by edestrians, from its extreme slipperyness in wet or icy weather. placing in constant danger the lives and limbs of our citizens, hould be forbidden by law; and also

Resolved, That the Engineers' Society of Western Pennsylvania earnestly petition Councils of the Cities of Pittsburgh and llegheny to adopt such ordinances as will remove all metal gratings and coverings from our pavements, and render it safe for edestrians in all weather.

Considerable opposition was made to the resolution, and no action was taken.

Mr. M. J. Becker then read the paper of the evening, entitled "Why do Rail Joints and Splice Bars Break?" In this paper, which was illustrated by drawings, the author confined himself to a description of that particular device of splice bar which has found most favor among railroad men, and which in its general form has now the most extensive application upon the leading railroads in the country. This splice is the so-called angle bar joint; it consists of two rolled bars, usually about 24 inches long, fitting closely under the head of the **T** rail and over its base, projecting just far enough outside of the foot of the rail to admit of receiving the slots for spiking the bars to the cross ties. The bars are punched for the bolts which pass through both angle bars and rail web: the punch holes for the inside bars being oblong to receive correspondingly shaped bolt heads, and thus prevent the bolts from turning while being screwed up to a bearing. The diameters of the bolt holes are slightly larger than the bolts, to allow for maximum expansion and contraction of the rail by extreme changes in temperature. The question, How are such beams affected by a load? is treated theoretically, by taking the rail as a beam fixed at one end and loaded at the other, and thus determining the deflection and the stress on the extreme fibre of the rail rection due to a load equal to the weight of a large driving wheel of a passenger engine and its carrying load. The modulus of elasticity of mild steel is taken at 27,000,000. The strength and deflection of the angle bars, loaded in the middle and supported at the end, is also considered theoretically, the same load being used.

After considering the angle bars and rail ends independently, the author next considers the combination, as in use, and determines the carrying capacity of each by assuming a given deflection. From these calculations the author finds that the body of the rail is nearly twice as strong as the angle bars, and fully fifty per cent. stronger than the projecting rail ends at the joints.

Again, from calculations the author finds that if intermediate ties are spaced $14\frac{1}{2}$ inchest apart, joint ties, in order to make the joints of equal strength with the body of the rail, should be spaced 8.84 inches apart. Splice bars break at the top, in the middle of the joint between the two interior bolt holes, and the rupture is a clean tear. Many joints break from defective metal. Very good results have been obtained of late by increasing the upper section of the bar. The author is inclined to adhere to his opinion, that the rupture of the angle bars is purely tensional, caused by the two rail ends acting successively as cantilevers on each half of the length of the angle bars, with a maximum strain at the middle of the bars.

WHY DO RAIL JOINTS AND SPLICE BARS BREAK?

[A paper read before the Engineers' Society of Western Pennsylvania, Jan. 20, 1885, by M. J. BECKER.]

When the first President of this Society presented to its members the first paper in the series of our periodical contributions, he framed its head lines in the interrogative mood, and affixed to its title an interrogation mark. The eminent propriety of this precedent has been amply acknowledged by subsequent contributors, and I trust that I too may be pardoned if I venture to present my paper this evening in the familiar shape of a conundrum.

When you desire information on a subject, of which you yourself are in ignorance, there is no better method of finding what you want to know than to read a paper and display your own want of knowledge regardless of all sacrifice of self-respect; this will encourage your audience to display unrestrainedly their own wisdom in the discussion, and will prove far more conducive to general enlightenment, than a too profuse exhibit of your own familiarty with your subject matter, which is apt to silence controversy and will cause your hearers to accept your own views as authority without question or dispute.

Fig.1

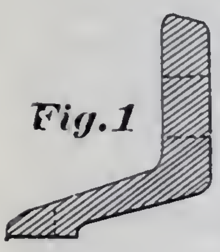
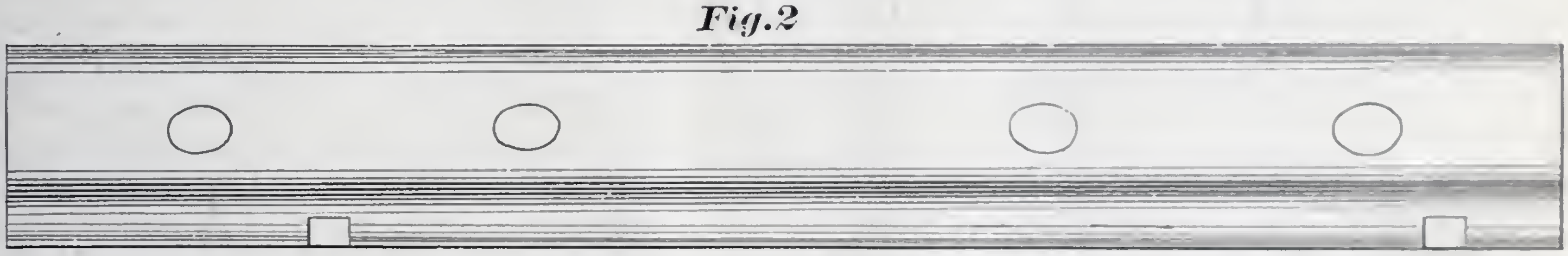


Fig.2



The subject upon which I would like to receive some information by means of the discussion which I trust the reading of this paper will bring forth, is this:

"Why do rail joints and splice bars break?"

This question must certainly seem simple enough, and no doubt finds a very ready answer in a general way to the effect, that if these splice bars and rail joints break, it is because they are incapable of resisting the strains which are brought upon them by the forces to which they are subjected. That is true enough as far as it goes; of course, when we know the magnitude and direction of any acting forces, we can readily determine their effect upon supports whose position and character are likewise known, and, knowing the effect of the acting forces upon their supports and also knowing the resisting capacity of the materials composing the supports, it would certainly seem that we ought to be able to devise their proper shape and determine the size necessary for the safe performance of the duty imposed.

Sometimes it happens, however, that the practical applications of theories apparently quite sound, produce results at variance with established principles which we have been accustomed to acknowledge as laws. In such cases a retrogressive force of reasoning from effect to cause, comes properly into play.

The splicing together of the rails of railway tracks has been accomplished by various means during the successive periods of railway construction. Commencing with the crude old fashioned wrought iron lip chair, these fastenings have undergone many changes and improvements, gradually tending towards a more rigid support and a more secure connection of the rail ends, resulting thereby in greater security and comfort to travel and in a less ruinous destruction of rolling stock. To refer even briefly to the endless variety of patented and unpatented contrivances and appliances invented for this purpose would consume more time than the most tolerant and patient of my hearers could be expected to concede.

I will therefore confine myself to a mere description of that particular device, which has of late found most favor among railroad men, and which in its general form has now the most extensive application upon the leading railroads of the country, varying of course, here and there, in some minor and unimportant particulars.

This splice is the so-called angle bar joint; it

Fig. 3

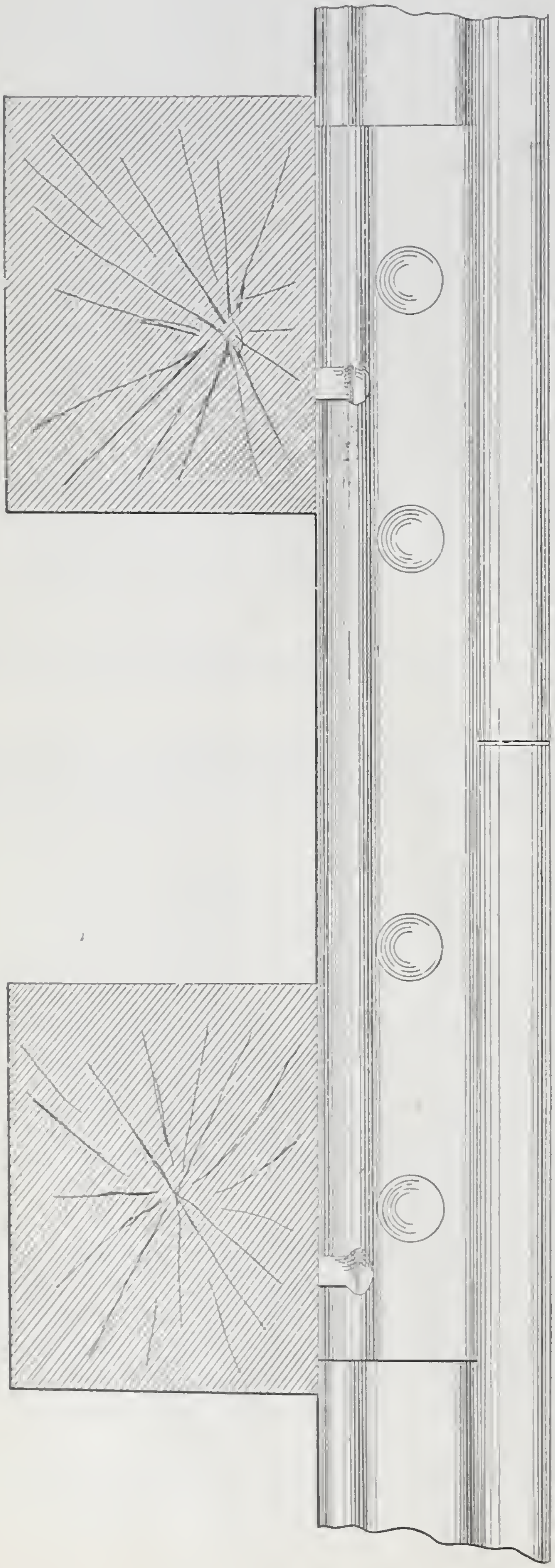


Fig. 4

consists of two rolled bars of the form shown on the sketch, Figs. 1 and 2, usually about 24 inches long, fitting closely under the head of the T rail and over its base, projecting just far enough outside of the foot of the rail to admit of receiving the slots for spiking the bars to the cross ties. The bars are punched for the bolts which pass through both angle bars and rail web, the punch holes for the inside bars being oblong to receive correspondingly shaped bolt heads and thus prevent the bolts from turning while being screwed up to a bearing; the diameters of the bolt holes are slightly larger than the bolts to allow for the maximum expansion and contraction of the rail by the extreme changes of temperature without shearing the bolts. A simple and very effective contrivance known as the Verona Nut Lock, consisting of a steel ring, cut through slantwise, so as to leave sharp knife edges at the cut ends, coiled and tempered to a spring of about 1,500 pounds elastic resisting capacity, is usually interposed as a washer between the angle bar and the nut of the bolt which fastens the angle bar to the rail. This washer has obtained a very extensive application throughout the country, and, although not an absolutely perfect lock against turning and liable to lose its elasticity in course of time, it appears to answer the purpose as well as any other devise heretofore employed, while its cheapness and simplicity of application present advantages which no other appliances known to me appear to possess.

The complete splice as above described is shown on the sketch, figures 3 and 4; this is the splice used as a standard on the lines of the Pennsylvania Railroad System, and is the one to which my observations, of which I will speak directly, have been chiefly confined. As applied on the lines above named, it is a so-called "suspended joint," the rail ends meeting midway between two cross-ties, and the ends of the angle bars resting upon the two joint-ties, forming to all intents and purposes a compound beam resting upon the two end supports with the bars, and the whole united together by the bolts.

Now how would such a beam be effected by a load?

To determine the effect of an external force upon the different component parts of a composite body is attended with but little difficulty, if the parts are uniform, equally affected and homogeneous in their character, but the difficulty increases as the relations and connections of the component parts of the body become more intricate among themselves and more at variance with each other. In order to determine the effect of a given load upon a compound beam composed of the two ends of adjoining T-rails, spliced and

bolted together by two splice bars in the manner above described, it would seem rational to investigate first the effect of the load upon the rail ends, independent of the splice bars; then ascertain the effect of the load upon the splice bars, independent of the rail ends, and then determine whether the one or the other of the members alone is affected by the load, or whether both are affected and in what ratio to each other, or whether the combination of the members affects the resisting capacity of either, and if so, to what extent.

Taking for an illustration the section of an ordinary T-rail of the Pennsylvania Railroad standard pattern, weighing 69 pounds per yard, we find that its sectional area = 6.81 square inches, that its centre of gravity lies almost exactly in the middle of its height and that its moment of inertia is = 17.43. Spacing the cross ties so as to distribute 16 ties to a 30 foot rail, they will then be 22½ inches apart between centres, and allowing 8 inches top face for each tie, the clear span of the unsupported rail equals 14½ inches, and the projecting part from edge of support to the rail end at a splice is 7¼ inches; if the driving wheels of one of the large passenger engines, with its weight of 20,000 pounds, rests at the tip of the rail end, unsupported by the splice bars, and the other end is considered fixed, or, what is very nearly the same thing, continuous over the end support, the effect of the load would be to press the tip end of the rail, with a tendency to rupture the rail at the point of support over the edge of the cross-tie, by tearing apart the fibres of the top flange, and crushing the fibres of the bottom flange of the rail; in other words, the maximum bending moment under the conditions stated would occur at the point of support and would be equal to the load multiplied by the distance between the load and supporting point; $M = W\,l = (20{,}000 \times 7\frac{1}{4}) = 145{,}000$ pounds, and the stress in extreme fibres of the rail section would be equal to $S = \dfrac{M}{\frac{I}{e}}$, in which, S = fibre strain per square inch, I = moment of inertia, e = distance of neutral axis from extreme fibres,—making $S = 18{,}300$ pounds. The actual transverse strength of the projecting rail, assuming maximum fibre strain at crippling point to be 45,000 pounds, = 49,175 pounds.

The deflection due to this load would be $\dfrac{W\,l^3}{3\,E\,I}$ = 0.0054 inches, in which formula, E = modulus of elasticity of mild steel, assumed at 27,000,000, being the average result of fifteen experiments made recently by the Pencoyd Iron Company on mild steel deck beams and I beams. (See James Christie's paper on strength and elasticity of structural steel in August number of Transactions American Society Civil Engineers.)

If we consider the engine wheel, with its weight of 20,000 pounds, so placed as to rest directly over the splice, and one-half of its weight carried by the tip of one rail and the other half by the tip of the other rail, then the maximum bending moment would be one-half the amount ascertained above, or 72,500 pounds; the stress on the extreme fibres would be 9,150 pounds and the corresponding deflection at the tip end of each rail would be 0.0027 inches.

You will observe that these figures are derived from the theoretical effects of purely static, quiescent loads, and would not, of course, compare at all, with the practical effects of moving loads with their accompanying impact; but for the present purpose, of investigating the comparative transverse strength of the component parts of the rail splice, the conditions above assumed will answer. So much then for the rail part of the joint.

To ascertain next the effect of the wheel load upon the two angle bars, considering them firmly united to each other, and doing equal duty, the wheel resting in the middle of the bars, and the bars standing in their normal position as if spliced but disconnected from the rail, is somewhat more complicated than the determination of the resisting capacities of the rail section.

A mere glance at the shape of the angle bars, and their position in the completed joint, will suffice to show that the effective capacity of the bars to resist a vertical load applied in the middle of their length cannot be determined from the elements which enter into similar solutions in bodies of symmetrical sections, with their centres of gravity, neutral axis and centres of gyration on lines normal and at right angles to the direction of the load. Neither can it be assumed that all parts of the angle bar section would be effective under the usual methods for determining the strength of symmetrical bodies. Take for instance a case slightly exaggerated, but still illustrating the point in question; if the two legs of the angle bars were of equal length and thickness and inclined to each other exactly at right angles, one leg standing perpendicular and the other resting with its entire surface upon a horizontal support; the centre of gravity of the bar would in such a case be outside of its cross-section, in the angle between the two legs.

Now if a load were placed on top of the vertical leg of the angle, its effect upon the extreme fibres of both legs certainly would not be equal, although the fibres are equi-distant from the geometrical neutral axis; indeed it is very evident that the horizontal leg does not carry any considerable part of the load and that the part

of its section outside of the prolongation of the vertical leg is not affected to any great extent, if at all, although the surplus metal contained in the horizontal leg may increase the tensile strength of the lower part of the section, as against the compressive strength of its upper part, where no corresponding surplus metal exists. In other words the *geometrical* neutral axis of the bar section is not the proper datum line from which to deduce its strength; but that another line, which may be called the *static* neutral axis, is much more nearly the datum line for the development of the moments and stresses due to vertical loads.

But referring again to the sketch it will be noticed that, owing to the peculiar attachment of the bars to the rail, the transmission of the load through the rail head by way of the inclined contact line under the rail head, and the retransmission from the angle bar to the top of the inclined rail base, the direction of the thrust is somewhat problematical; it certainly is not vertical, but its vertical origin is decomposed into two oblique forces, the direction and magnitude of which I have endeavored to locate by the following methods:

The location of the neutral axis was ascertained by balancing a brass templet of the angle bar section over the edge of a knife blade, taking care to maintain the knife edge parallel to the bottom line of the templet. The moment of inertia about this horizontal axis was then calculated in the usual way, and from it the radius of gyration.

To locate the centres of inertia upon the platted cross-section of the bar, the respective centres of gravity of the portions above and below the neutral axis were determined, and a line drawn through these centres of gravity.

The centres of inertia were taken on this line at a perpendicular distance from the neutral axis equal to the radius of gyration. This result is shown in figure 5.

The two centres of inertia are not in the same vertical line, and the line joining them is *inclined* to the vertical, and therefore also inclined to the line of action of the load.

The resistances of the upper and lower segments, with reference to a vertical load and horizontal axis, are not in the same plane, and if the bar were not supported laterally, it would rotate upon its base. But if supported laterally the resultant force *must* act along the inclined direction, and with two bars bolted upon the sides of the rail, we obtain the conditions shown by Fig. 6.

The force acting upon each angle bar will be the component of the vertical load, in the direction of the inclined line of resistance; and the resistance of the bar will not be represented by its moment of inertia about a horizontal axis, but by its moment about an axis at right angles to the inclined line and through the centre of gravity of the section. The value of *this* moment has therefore been calculated and is the one which has been used for calculating the strength and the deflection of the angle bar. The angle which this oblique line of resistance makes with the vertical and also its secant are shown on the figure marked 6.

The sectional area of one of the angle bars for a 69 pound rail is 2.671 square inches, its moment of inertia is 3.147, its radius of gyration is 1.086, and the angle between the vertical and the line of thrust as developed above is 35 degrees.

Considering the angle bars to be of iron, with an allowable fibre strain of 25,000 pounds per square inch, the calculated transverse strength of the two bars, disconnected from the rail, would equal a vertically applied load of 17,300 pounds, and the flexure under the above assumed load, in the middle of the bars, is 0.0074 inch. Steel angle bars of the same dimensions and under equal conditions, allowing 45,000 pounds fibre strain per square inch, will carry 28,850 pounds and deflect 0.007 inches.

If the method pursued in determining the properties of the angle bar is correct, we may then presume to know, as far as theoretical calculations permit, the transverse strength and deflection of the rail section alone, loaded at the joint, and the strength and deflection of the angle bars loaded in the middle independent of the rail; the next step will be to ascertain the combined strength of the two, when properly attached and united to each other, for in that manner the load will be applied in practice.

As shown above, the actual transverse strength of the projecting rail end is 49,175 pounds; its fibre strain, due to a wheel load of 20,000 pounds, is 18,300 pounds, and its end deflection is 0.0054 inch; the actual transverse strength of the angle bars is 17,300 pounds with a fibre strain, due to the wheel load, of 25,200 pounds for the top flange, and 27,800 pounds for the bottom flange. and a deflection, under equal load, of 0.0074 inch. The work done by that load, acting upon the completed splice, consists of bending two projecting rail ends and the pair of angle bars through a deflection distance, which, from the nature of the fastenings, must be common to all the members in the combination; and this equality of deflection affords the means of determining what proportion of the load is borne by the rails and by the splice bars respectively.

The general formulæ for deflections are:

For projecting beam loaded at the end—

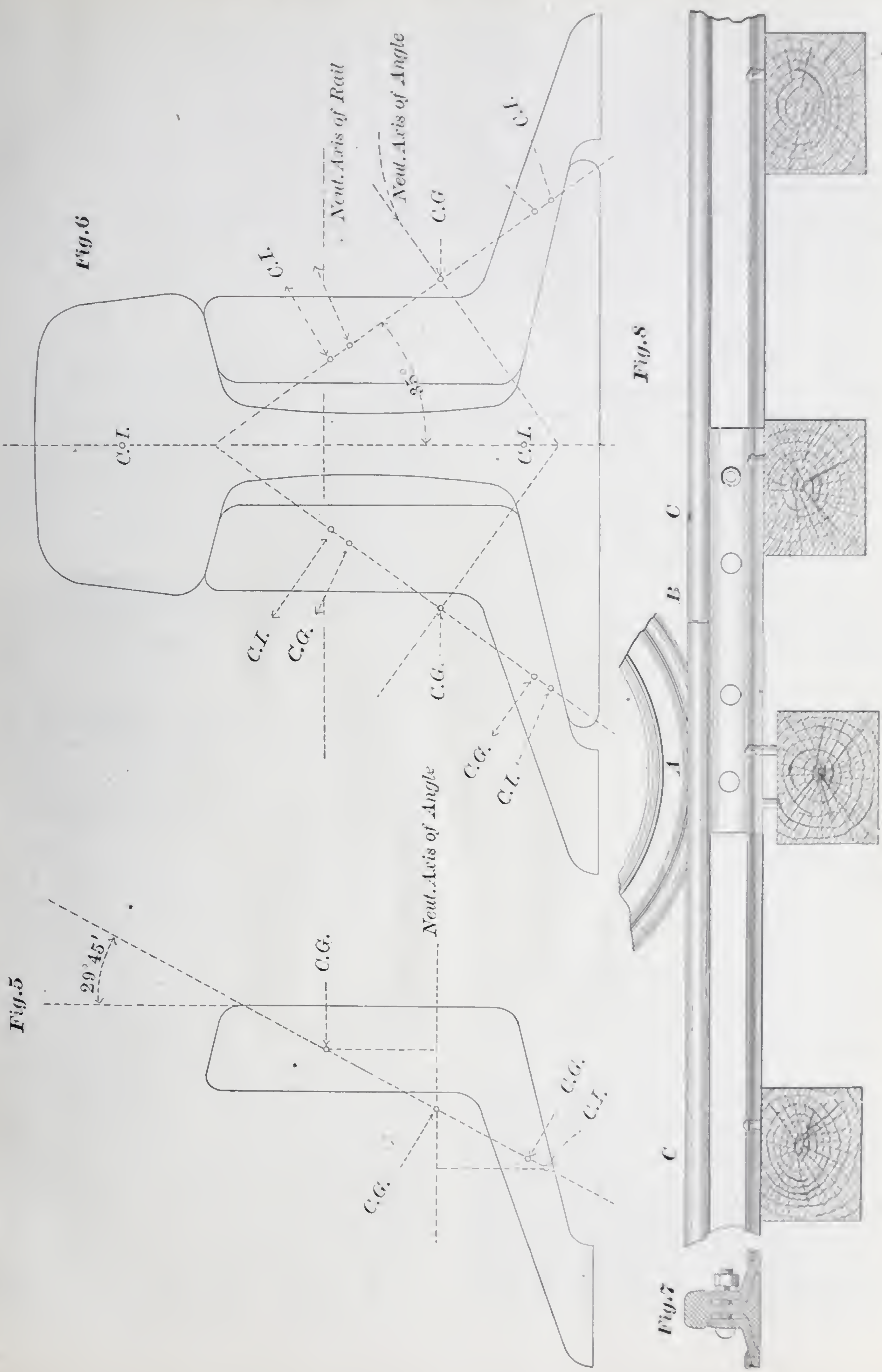


Fig.5
29°45'
C.G.
Neut.Axis of Angle
C.G.
C.I.
C.G.
Fig.6
C.I.
Neut.Axis of Rail
Neut.Axis of Angle
C.G
C.I.
35°
C.I.
C.I.
C.I.
C.G.
C.G.
C.G.
C.I.
Fig.8
A
B
C
C
Fig.7

$$d = \frac{W l^3}{3 E I} \ldots\ldots\ldots\ldots\ldots\ldots (a)$$

For beam on two supports loaded at the center—

$$d = \frac{W l^3}{48 E I} \ldots\ldots\ldots\ldots\ldots\ldots (b)$$

Let W = total wheel load.

Let w = that part of W expended in bending each projecting rail end.

Let w' = that part of W expended in bending the pair of angle bars.

Let I = moment of inertia of the rail section.

Let I' = moment of inertia of the two angle bars.

Let L = clear span between joint ties.

Let $\frac{L}{2} = l$ for rail ends.

Let l' = clear span between intermediate ties.

By substituting these values in equation (*a*) and (*b*) we have:

For each rail end—

$$d = \frac{w\left(\frac{L}{2}\right)^3}{3 E I} = \frac{w L^3}{24 E I} \ldots\ldots\ldots\ldots (1)$$

And for a pair of angle bars—

$$d = \frac{w' L^3}{48 E I'} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2)$$

The deflections being alike for both we may write—

$$\frac{w L^3}{24 E I} = \frac{w' L^3}{48 E I'} \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

By cancellation this becomes—

$$\frac{w}{I} = \frac{w'}{2 I'},$$

Or, $$\frac{2 w}{I} = \frac{w'}{I'},$$

Which may be written, $2w : w' = I : I' \ldots\ldots (4)$

From which also $2w : 2w + w' = I : I + I'$

But $2w + w' = W$;

and by substituting, $2w : W = I : I + I'$,

from which $w = \frac{W}{2} \frac{I + I'}{I} \ldots\ldots (5)$

Recurring to (4) and taking the sum of the terms in the reverse order we have—

$$2w + w' : w' = I + I' : I' \text{ or } W : w' = I + I' : I',$$

from which, $w' = W \frac{I'}{I + I'} \ldots\ldots\ldots\ldots\ldots\ldots (6)$

These two equations, (5) and (6), determine the proportions in which the load is distributed to the several members composing the joint. In other words, the total load W, so applied as to test equally upon the two rail ends, and transmitting its force through the rail ends upon the angle bars, in such manner as to produce equal deflections in the rails and angle bars, is decomposed into the forces w and w', the former acting through the rails and the latter through the angle bars.

Now if figures tell the truth, the stress on the flange fibres of the angle bars, produced by that part of the total load W which causes the deflection in the bars equal to the deflection of the rail, $= w' = \frac{W I'}{I + I'}$ and $S = \frac{W L e \, Sec. a}{4 (I + I')} = \frac{20{,}000 \times 14.5 \times 2.2 \times 1.22}{4 \, (17.43 + 6.29)} = 8{,}190$ pounds.....(7)

and the stress on the flange fibres of the projecting rail end, produced by that part of the load W which causes a deflection in the rail ends equal to that of the angle bars, $= w = \frac{W}{2} \times \frac{I}{I + I'}$, and $S = \frac{W L e}{4 \, (I + I')} = 6{,}724$ pounds...(8)

These figures would bring the fibre strains in angle bars and rails within the limits usually allowed for safety, even if we allow an addition of 50 per cent to the load for impact, but when the uncertain effects of lateral shocks and the centrifugal force of rapidly moving trains are added, the exterior angle bars on the outer rail of the curve would likely be found deficient in safe strength, if not actually strained beyond the limits of elasticity.

The fibre strain of a portion of rail resting upon two intermediate cross-ties, loaded in the middle, $= S = \frac{1}{4} \frac{W L}{\frac{I}{e}}$, or 9,150 pounds, and, if the rail is considered fixed at the supports, which, on account of its continuity, is more nearly its actual condition, the fibre stress is only 4,575 pounds, showing that the body of the rail is nearly twice as strong as the angle bars and fully 50 per cent. stronger than the projecting rail ends at the joints.

Referring again to formulæ 7 and 8, we notice that the fibre stress in the projecting rail ends, due to that part of the load W which produces the equal deflections in rail ends and angle bars, $S = \frac{W L e}{4 \, (I + I')}$; and that the fibre stress in the angles due to that part of the load W, producing its effect upon the splice bars, $S' = \frac{W L e' \, sec. \, a.}{4 \, (I + I')}$

Comparing these equations, we see that if the rails and splices were of equal metal, they would be of equal strength if $e = e' \, sec. \, a$.

But the allowed maximum fibre strain, S, in steel is 45,000 pounds, while that for iron, S', is only 25,000 pounds per square inch, both figures applying to the limit at which crippling takes place.

From this, $S' = \frac{5}{9} S$.

For rail on intermediate ties, $S = \frac{W \, l \, e.}{4 \, I}$

$$\frac{5}{9} S = \frac{\frac{5}{9} W \, l \, e}{4 \, I} = \frac{W \, L \, e' \, sec. \, a}{4 \, (I + I')};$$

$$\text{or, } \frac{5\ l\ e}{9\ I} = \frac{L\ e'\ sec.\ a}{(I + I')}.$$

$$L = \frac{5\ l\ (I + I')\ e}{9\ e'\ sec.\ a\ I}$$

For 67-pound rail, this is equal to $L = l \times 0.61$; or, in other words, if intermediate ties are spaced $14\frac{1}{2}$ inches apart, joint ties, in order to make the joints of equal strength with the body of the rail, should be spaced only 8.84 inches apart.

This corresponds exactly with the experiments made by Baron von Weber, Minister of Public Works of Prussia, in which he measured, by registering scales placed under the rails, the different deflections, caused by the passing loads upon rails between joint-ties and between intermediate ties, and from which observations he established the general rule, that, with the ordinary angle bar splices, joint-ties should be placed 0.6 the distance of intermediate ties. [See his report in London *Engineering*, vol. X, page 294.]

It is not presumed that the foregoing figures are intended to show the actual strength of the splice joints, or even give an approximate approach thereto, but they cannot fail to demonstrate the relative weakness of the joints as compared with the body of the rail. Add to this inherent weakness the many causes tending to aggravate the case, such as the imperfections in the track, even under the most careful supervision flat wheels, worn ties, rough wheel flanges, loose bolts and rough surfaces of contact of the parts composing the rail joints, and you may well say, "No wonder that they break." Of course, these aggravating causes apply, at least to a certain extent, to the other portions of the track, but they exert themselves more effectively in the weaker spots, and the fact still remains that the joint is the weak spot in the track, and, since the greatest strength of any structure is only that of its weakest part, it seems strange that so little has been done by the profession to investigate this subject and improve the existing defects; and when, in addition to what I have said to you on the theoretical weakness of these joints, I tell you that I have found 1,130 broken angle bars on a single division of six miles of track, you will naturally be surprised; and I confess that I was quite startled by the discovery.

On six miles of track, counting the rails to be of 30 feet length, there are 2,112 joints, or 4,224 angle bars, of which 27 per cent. were found broken; but, strange to say, every one was broken on the top, in the middle of the joint, between the two interior bolt holes, the fracture extending from the outer edge downwards in lengths varying from $\frac{1}{4}$ inch to the total width of the vertical leg of the angle bar, but in not a single case did the fracture extend into the bottom flange or horizontal leg of the angle bar, and the rupture was in every case a clean tear, evidently caused by extension of the upper fibres of the top flange.

You will see, therefore, that these fixtures do not break according to the rules of the books. They defy your statical moment and sneer at your modulus of resistance. They break most numerously when entirely new, just after being laid, notwithstanding the most perfect support and most careful adjustment. They break everywhere, although they do it most persistently on gravel ballast, but, in the absence of any ballast, they break as well; indeed, they seem to have a preference for the smooth and perfectly built floors of iron bridges and viaducts.

It is undoubtedly true that a large share of this destruction of joints is due to the defective metal in the bars. I never had any analysis made of the material, and I cannot definitely say to what extent furnace cinder preponderates in the composition. Some iron manufacturers (present company, of course, excepted) do sell some very inferior metal to railroad companies occasionally, just for the purpose of showing how easy it is for them to deceive the young graduate from the Polytechnic Institute, who is learning the practical part of the profession at the expense of the railroad company while serving with the title of "Inspector" at a rolling mill.

These failures, however, did not occur with angle bars of the sections shown in the preceding figures, the dimensions of which have been used in the foregoing calculations; they occurred with bars, a section of which is shown in Fig. 7. The present standard section is slightly increased, and it is proper to state that thus far no failures have been reported of the new bars, of which a small number have been recently laid.

The original advocates of the so-called suspended rail joint never anticipated such results, nor did its opponents ever point them out. The effect is due to causes not looked for by either side. What, then, is causing the mischief, and what is its remedy?

When we find a piece of iron bent downwards at the ends, with the upper fibres pulled apart in the middle and the lower fibres crushed, we know that either one or the other of the ends was bent downwards over a middle support, while the other end was being held in position, or that both ends were bent down together. Again, we know that the construction of a projecting beam of uniform strength throughout its length must be such that each cross-section, no matter where taken, is of such area as to satisfy the bending moment produced by the load. This would imply a structure similar to the gradual re-enforcements from projecting end to supported part, in a spring with its successive lay-

ers of leaves, or the increasing diameters of the tubes of a sliding telescope. If any section be of less area than that required to satisfy the bending moment of the load, rupture would take place at that point.

Now, it must be admitted that, although the action of the forces affecting a rail joint is different from those assumed in the foregoing calculations, there is still ample proof that the joint is really the weak spot in the track. This weakness naturally admits of a greater movement and vibration under passing loads at the joint and its adjacent supports than at any intermediate points. Standing alongside of a railroad track when a train passes over it, this fact becomes at once apparent to the observer. It is very noticeable during a rain, on a line ballasted with fine gravel or sand, when the disturbance of the joint-ties will cause water to collect under them during the upward movement, which water is again forced out by the downward movement, cutting well-defined gutters from the ends of the joint-ties down the ballast slopes, while no such effect is noticeable at the intermediate ties. I have even observed similar evidence on the sand-ballasted railroads of the western plains, during high winds, which blow away the ballast from the ends of the joint-ties, where it is agitated by their movement under the passage of trains, while no such effect is produced elsewhere along the track.

Unless these joint-ties receive extraordinary special attention, on the part of the trackmen, their neglected condition will inevitably cause the troubles above related. Indeed, I doubt very much whether any amount of care would prevent the natural consequences of a defect inherent in the present arrangement.

Suppose the ballast under the joint-tie *A*, Fig. 8, to be washed out to the extent indicated, while the joint-tie C is undermined to a less degree or not at all. When the heavy driving wheel of an engine strikes the tie *A*, the deflection produced by the load would cause bending, or probably rupture at *C*, if the beam (rail and splice combined) were of uniform strength throughout; but the rail being severed at *B*, the strength of that part of the beam between *A* and *B* consists of the angle bars alone, and if the capacity of the angle bars to resist the moment of the load and its leverage *A B* is less than the united capacity of the rail and angle bars to resist the moment of the load and its leverage, *A C*, then rupture must take place at *B*. The fibre strain in the angle bars at *B* is found by calculation to be almost double that of the fibre strain in rail and angles combined at *C*, and rupture at *B* would therefore be inevitable under the circumstances, if the load at *A* and its impact are sufficient to produce it anywhere.

There is upon any railroad track, irrespective of its support, and noticeable even upon the most rigidly ballasted road-bed, a wavelike motion, preceding every approaching train, conforming, in an irregular manner perhaps, to the rise and deflection under a passing load of a continuous beam resting upon several supports. This wave would undoubtedly terminate in an upward movement at any joint between two supports, where the continuity of the beam ceases; and some theorists have attributed the ruptures in the upper flanges of the angle bars to this cause but the undulations caused by this wave-like movement are too insignificant to produce such an effect, if the supports all maintain their fixed positions in the plane.

Until convinced by better proof, I am inclined to adhere to my opinion that the rupture of the angle bars is purely tensional, caused by the two rail ends acting successively as cantilevers on each half of the length of the angle bars, with a maximum strain at the middle of the bars.

This action is, moreover, aggravated whenever, by wear or slackening of the nuts or bolts, all surfaces of the joint connection are no longer in perfect parallelism; the movement tending to break the bars is twice repeated by the passage of every wheel, first, as it approaches the leading tie, causing that to be depressed (even in the best stone ballast) for a certain distance, while the receiving tie, with its rail end and its half of the angle bar, remains in another plane, its normal position; evidently, being bolted together, the one end loaded, the other free, there must be a bending moment on the bars which connect them equal to the difference in actual level, however small, of the leading and receiving ties into the load, and aggravated by the momentum and many incidental causes, the next instant this movement is reversed; now the receiving tie has the load and the first leading tie, with its rail end, is free, its own elasticity and that of the rail tending to raise it to its former normal position. In both movements the bars receive such a strain as to produce, after a number of repetitions, the rupture above described.

This solution seems sufficiently satisfactory to me as far as the breakages on ballasted track are concerned. As to that part of the conundrum relating to the breakages on perfectly floored bridges and viaducts, I am constrained to follow the well-known example of that much traduced animal that tried to join its mate on the opposite shore without crossing the stream—I give it up.

For the purpose of comparing theory with practice, I give you below the results of seven tests made by Mr. T. Rodd, under the supervision of Mr. Onward Bates, both members of this Society, of the transverse strength of several

types of rail joints in use in 1876. These tests were made at the Keystone Bridge Works. Steel rail ends of 60 pounds weight per yard, and of the standard section used by the Pennsylvania Company at that time, were spliced and bolted together in the usual manner, successively, with straight splices; with straight splice on one side and angle bar on the other; with double angle bars of Pennsylvania railroad pattern; with double angle bars of Pennsylvania Company pattern; with two straight Samson bars; with straight Samson bar on one side and angle Samson bar on the other, and with double Samson bars. A test was also made of the transverse strength of a solid piece of 60-pound steel rail twenty-four inches long. The piston pressure was applied in the middle of the lengths of the spliced end of the rail section with the following results. The series embraced eight experiments.

No.	Description.	Length.	Weight.
1..	P. R. R. standard 2 straight bars.	24 inches.	18 lbs.
2..	" " 1 " and 1 angle	24 "	27 "
3..	P. R. R. standard 2 angles	24 "	35 "
4..	Penna. Co. standard 2 angles	24 "	29¾ "
5..	Samson joint 2 straight bars	24 "	20 "
6..	" " 1 " and 1 angle.	24 "	28½ "
7..	" " 2 angles	24 "	26½ "
8..	Steel rail 60 lbs. per yard	......	......

The tests were made on bearings twenty inches apart, with results as follows:

In test No. 8, the solid rail, 60 pounds P. R. R. standard, showed first permanent set of 1-64 inch at 97,500 pounds load, and at 182,000 it failed by splitting under the head.

The Samson bar is a modification of the fish bar and the angle bar, consisting in a reduction of the sectional area near the ends of the bars to save metal and a very slight increase over the sectional areas of the fish and angle bars in the middle.

The tests show that while the straight Samson bar is somewhat stronger than the straight fish bar, the Samson angle bar presents no advantages over the regular standard angle bars. The tests of the joints composed of one straight bar and one angle give, as may be expected, rather unsatisfactory results.

The strength of the solid rail, as developed by the test, is more than double the strength of the angle bar joint, which proves the correctness of the theoretical calculations.

Upon several lines of railway, angle bars are used in the manner first above described, but the joint is spported by a cross-tie directly under the rail ends in the middle of the splice bars, as shown in Fig. 9. Such a joint would seem exposed to danger from two sources: In case the first tie from the joint is low, the load would still tend to rupture the angle bar on the top flange by tension, the load at A acting with a leverage *A B*, the edge of the joint tie, *B*, being the ful-

TESTS FOR TRANSVERSE STRENGTH OF RAIL JOINTS.

		Deflection under Successive Loads.										Ultimate		Remarks.
		10,000.		15,000.		20,000.		25,000.		30,000.				
	No.	Deflection.	Permanent set.	Deflection.	Permanent set.	Deflection.	Permanent set.	Deflection.	Permanent set.	Deflection.	Permanent set.	Load.	Deflection.	
Fish.	1..	1/16	0	1/16	0	5/64	0	3/32	0	9/32	¼"	50,960	3"	Broke 1 bar.
Fish and angle	2..	3/64	0	3/32	0	⅛	+	9/64	1/32	5/32	1/16	60,060	2 1/16"	" 2 bolts.
Penna. R. R. angle	3..	0	0	1/32	0	1/16	0	5/64	1/100	7/64	1/64	79,300		" " "
Penna. Co. angle	4..	1/64	0	3/64	0	1/16	0	1/16 +	0	7/64	1/32	83,720	3"	" " "
Samson fish.	5..	3/64	0	3/32	+	7/64	1/32	9/64	1/32			76,700	3 ⅞"	" " "
Samson fish and angle..	6..	3/64	0	1/16	1/64	3/64	1/32 +	9/64	3/64			74,700	3 ½"	" " " 2½" lat. defl. of angle.
Double Samson angle..	7..	1/32	0	3/32	1/100	5/64	1/64	5/64 —	1/64	⅛	1/64	78,000	2 ¾"	Broke 1 angle some lat. defl. of sound angle.

crum, a slight upward movement must occur at the rail ends every time a wheel passes from C to B over the depressed tie, A, tending to rupture the angle bars at B; and, in case the joint-tie itself is low, the load at B would tend to compress the top flanges of the bars with a force increased by the lengthened span between supports.

Angle bars have also been introduced of sufficient length to extend over three ties, the rail ends meeting over the middle tie; this evidently stiffens the unsupported portions of the rail between the middle tie and two adjacent ties and overcomes the tendency to rupture of the upper bar flange at the joint, but it does not relieve the two other troubles mentioned in the last case, and I doubt whether the additional cost of the joint is compensated by the advantages it may possibly afford.

Still another form of splice is coming into use, a section of which is shown in Fig. 10. If the breakages heretofore described would have been avoided by a larger top flange area, then this new section of angle seems to afford a remedy; to what extent its sharp re-entering angles may complicate the rolling process, especially in steel bars, some practical member may be able to say.

The so-called "Fisher Joint" is recommended and endorsed by engineers who have tried it in the track; it consists of a "U"-shaped trough, of a width to fit the base of the rail, and of a length of about twenty-one inches, the ends resting upon two joint ties, the rail being secured by U-shaped bolts, passing under the splice and fastened by nuts to the rail base, which is checked or slotted for that purpose; the devise is shown in Fig. 11. The trough certainly prevents deflection of the rail ends, and its sides confine the rails against lateral displacement, but its introduction meets with opposition on account of the radical changes it involves, embracing not only the slotting and punching of the rails, but the necessary checking down of the joint-ties to receive the plates.

The best joint is the one that accomplishes the purpose fully at the least cost. The economical limit for a practical and safe joint seems to have been reached in the standard angle bar above described, and if the breakage can be avoided by a slight increase in the section, as indicated in the standards of 1884, and by the careful selection of good iron or, if necessary, by the adoption of mild steel, I would consider the problem satisfactorily solved as far as actual danger from weakness of the joints is concerned. The increased wear of the connecting parts, and the consequent irregularity produced in the movement of trains over the joints, will not be over-

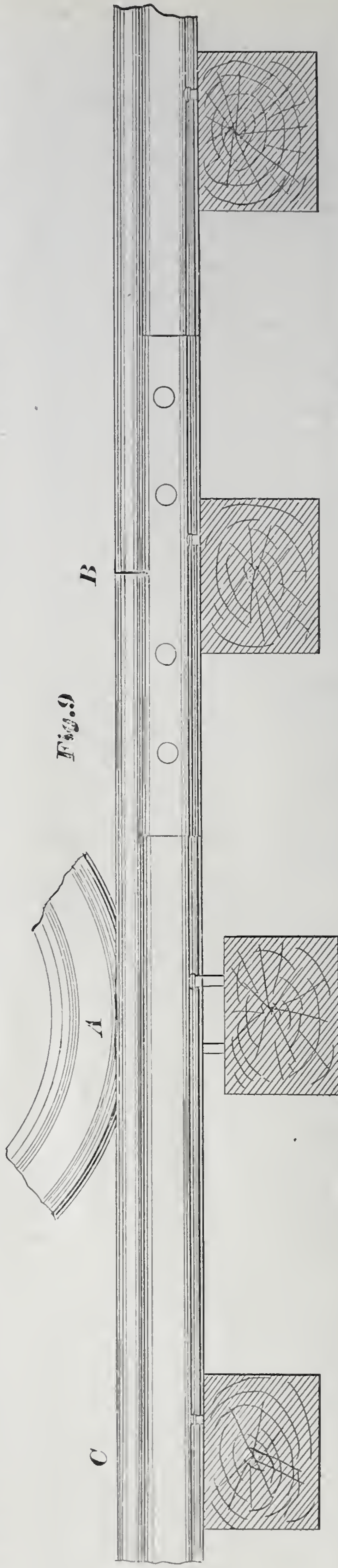


Fig. 9

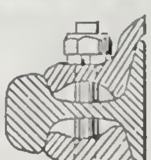


Fig. 10

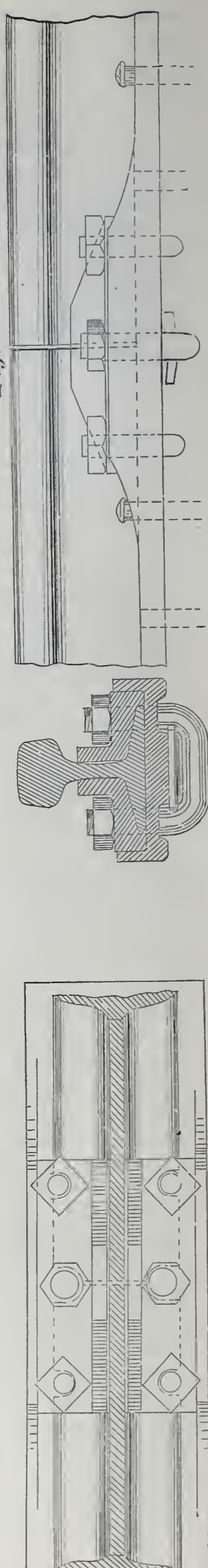


Fig. 11

come until a joint is invented as perfect as the unbroken rail, which amounts to an impossibility.

In conclusion, allow me to apologize for presenting to you a subject of so little general interest, the consideration of which, if it should receive any at all, must be confined to the few unfortunates, who share my own unenviable lot. But the profession is gradually disintegrating into specialties, and the pursuit of one branch of knowledge is becoming enough for one man; indeed, I have found it sometimes to be too much.

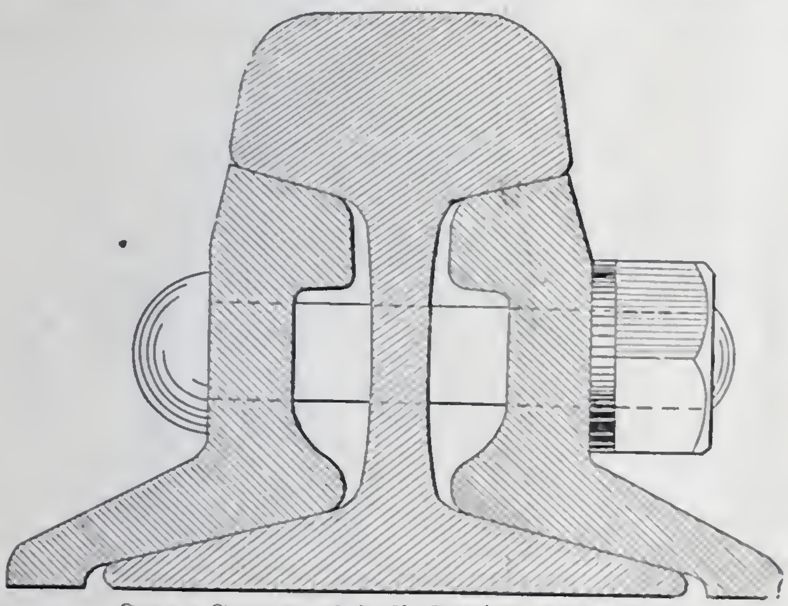
The universal genius is becoming scarce in this world, and we must content ourselves to give and receive such contributions as our limited individual stock in trade will afford.

Perhaps, after all, some of you never knew that splice bars would break, and you might have been killed on a railroad from the effects of a broken joint and never have known the cause. Of course, to a dead man, this would not be a matter of much consequence, but the knowledge that your untimely end was due to the distortion of the radius of gyration in a rail section, or to the buckled-up moment of inertia in a splice bar, might, perhaps, be a source of consolation to your grief-stricken widow.

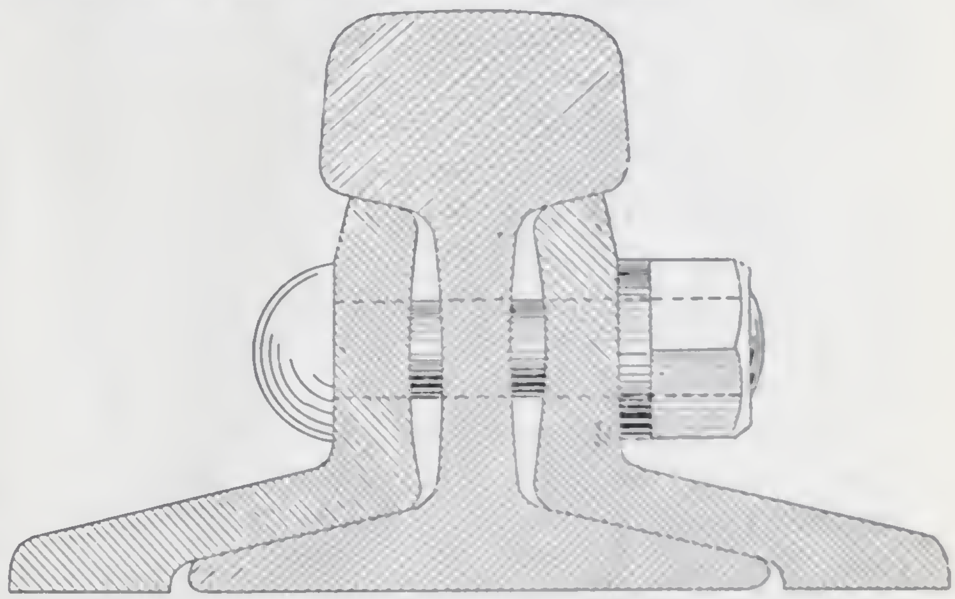
DISCUSSION, FEBRUARY 17.

Mr. Jones: I do not propose to offer any original ideas of my own on this subject, but propose to give the embodiment of the experience and practice of Robert H. Sayre, now Chief Engineer of the Southern Pennsylvania Railroad and lately Chief Engineer and General Manager of the Lehigh Valley Railroad. Mr. Sayre starts out with three questions to consider, First: The car wheel and its form; second, the rail and its form; third, the fish bar and its form.

Mr. Sayre from his observation and experience has decided on a standard for his car wheels as run over a straight track at a high rate of speed. The car with taper tread wheels showed considerable side vibration, while the car with the straight tread wheels ran with little, or scarcely any side vibration. This satisfied him thoroughly to abandon the use of taper tread wheels. It must be obvious to any mechanical mind, that there can be no utility in using taper tread wheels, as the difference in the peripheries must result in abrasion, if the whole surface of the top of the rail is in contact with the wheel; and if not in contact, then the whole load is borne by but a part of the rail and wheel.



Sayre Standard 67 lb. Rail & Splice-Bar.



Pennsylvania R.R. Standard 67 lb. Rail & Splice-Bar.

follows: On all wheels the tread is perfectly straight, with a $\frac{5}{8}''$ radius between tread and flange, the flange having an angle of 10°. Mr. Sayre unhesitatingly condemns (and I think justly so) the use of the taper tread wheel, which I think naturally originated with the pattern maker and moulder. To thoroughly test the superiority of the straight tread over the taper tread wheel, two cars were carefully constructed. One was fitted with straight tread and the other with taper tread wheels but were the same in every other particular. These cars were

In deciding upon the radius, between flange and tread of wheel, Mr. Sayre was governed by the actual experience gained on the Lehigh Valley Railroad. The same in regard to the degree of angle adopted for flange. Having decided on the form of car wheel to be used, he then proceeded to design a rail, whose bearing surface should at once conform to the surface of the wheel, and not wait for months of travel over the road to get the wheels and rails as they should be at the start. His 67 pound rail in use

on the Lehigh Valley road is 2¼ inches wide at the points of intersection, of the sides of the rail head produced, with a horizontal line drawn across the top of the rail. The angle of the side of the rail heads corresponds with the angle of the flange of the wheel, (10°); and the radius, between tread and flange of wheel, corresponds with the radius between the top and side of rail. The angle of the under side of head and the angle of flanges are 14 degrees. At the intersection of side and underside of the head the radius is quite small (⅛″), so as to give as wide a bearing for the fish bar as possible.

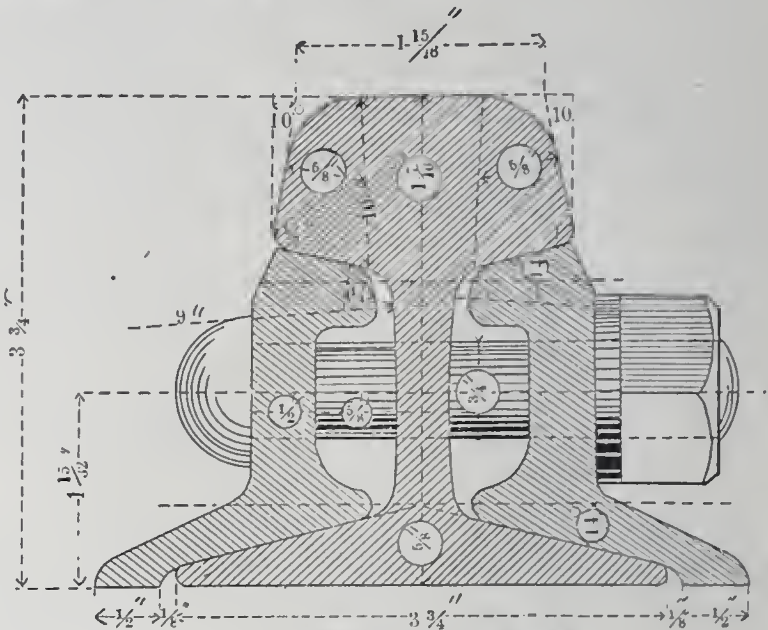
The construction of fish bar as shown in drawing, gives a good support to the head of the rail as well as good contact; while the form of the upper part of the fish bar, which is really a half section of an I beam, gives strength to that part of the fish bar which has heretofore been the weakest part, and is also sufficiently strong to prevent buckling as is frequently done in badly constructed joints.

All road masters know that the first fracture occurring in fish bars generally commences on the side of bar next to the top of rail, due no doubt to the change of fulcrum, while train is passing over joint. I do not propose nor attempt to elucidate why the fish bar first fails at that point. Suffice to say that we know it does so, and I should think it proper for the engineer to strengthen that weak point, which is generally known to be a very weak point, in railroad engineering. I think Mr. Sayre certainly overcomes this weakness as shown in the section of fish bar which he has designed.

Comparing Mr. Sayre's section of 67 pound rail with fish bar attached, with the P. R. R. standard 68 pound rail and fish bar, we find the contact surface between the fish bar and head of rail is ¾ inches, while the P. R. R. fish bar and rail only gives ⅜ inches, and is so weak that I am informed, it buckles when heavy trains pass over it, and it certainly is totally inadequate to perform the duty for which it was intended. I think the majority of engineers who design rail sections do not give the question of contact surface between fish bar and rail that attention it deserves. Complaints are frequent, especially from western roads, of rails battering down at the ends and the rail makers are at their wits end to overcome the evil.

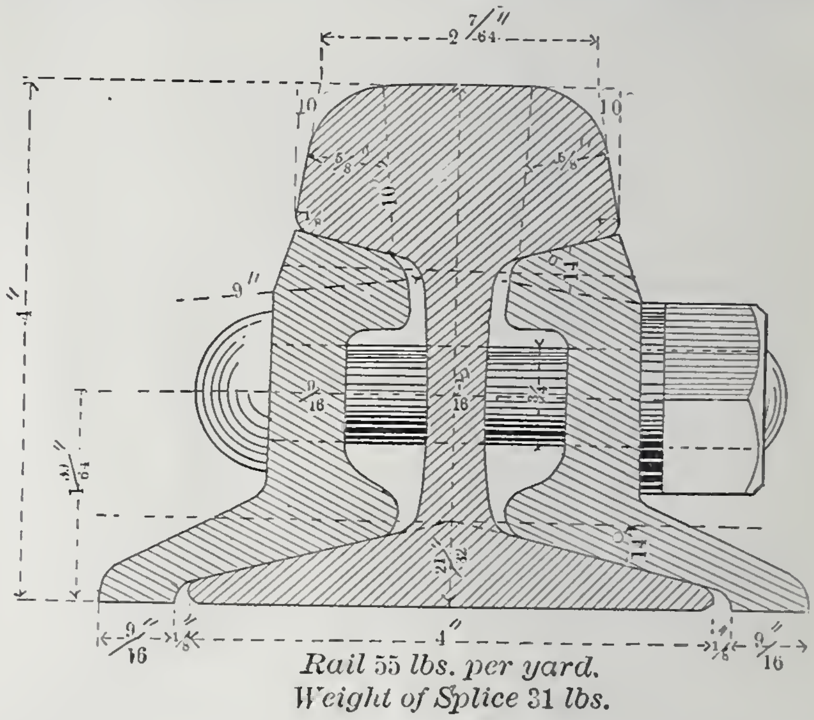
Rails have been made as hard as possible, compatible with safety, and they do not appear to do much better than soft ones; and thus rails wear well anywhere but at the joints. Yet engineers turn and energetically d—n the rail makers because they are unable to make rails hard enough to stand the pounding of high speed trains, over a weak and inefficient point, and yet be ductile enough to insure the safety of human life.

I submit for the inspection of members of this society drawings of six sections of rails, designed by Mr. Sayre, all embracing the same general ideas, embodied in his 67 pound section. [See this and following page.]



Rail 50 lbs. per yard:
Weight of Splice 25 lbs.

Now if our engineers could adopt a score of standard sections, they would certainly assist in materially reducing the cost of production, while at the same time they would be more apt to get rails with smooth surface, as each mill could afford to always keep duplicate rolls on hand at a small expense compared with the vast array of rolls that now adorn every spare corner of our

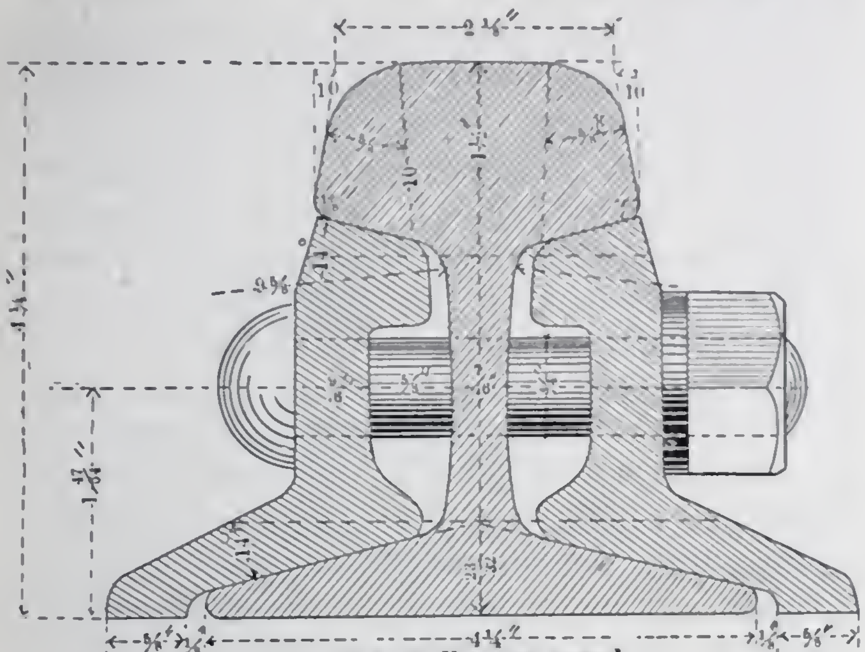


Rail 55 lbs. per yard.
Weight of Splice 31 lbs.

rail mills. I find in our section book, ten different sections of rails weighing from 59 to 60½ pounds per yard and not one section has enough contact surface under the head to admit of a fish bar thicker than ½ inch, and some of these vary so slightly that it requires great care to see that we do not roll rails off of the wrong rolls.

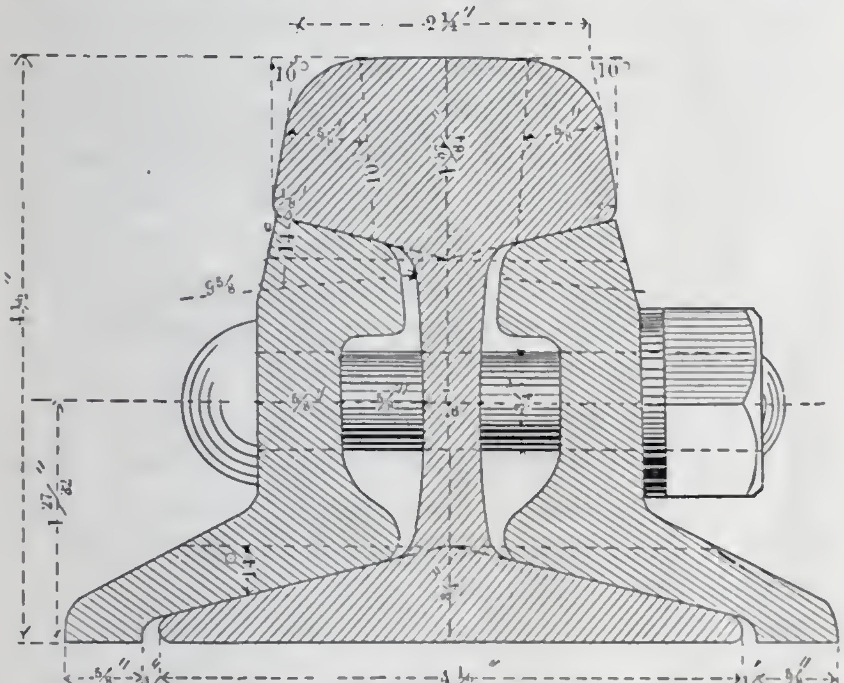
Mr. Sayre, anxious to avoid the oblong holes generally punched in fish bars, has seriously considered the use of the heavy grip bolt for use on the tracks of the Southern Pennsylvania rail-

road and is now discussing the use of steel for fish bars in place of iron. In conversation I recommended to have the holes in fish bars drilled instead of punched, and to slot half circle holes $\frac{5}{16}$" diameter to prevent bolt from turning; the



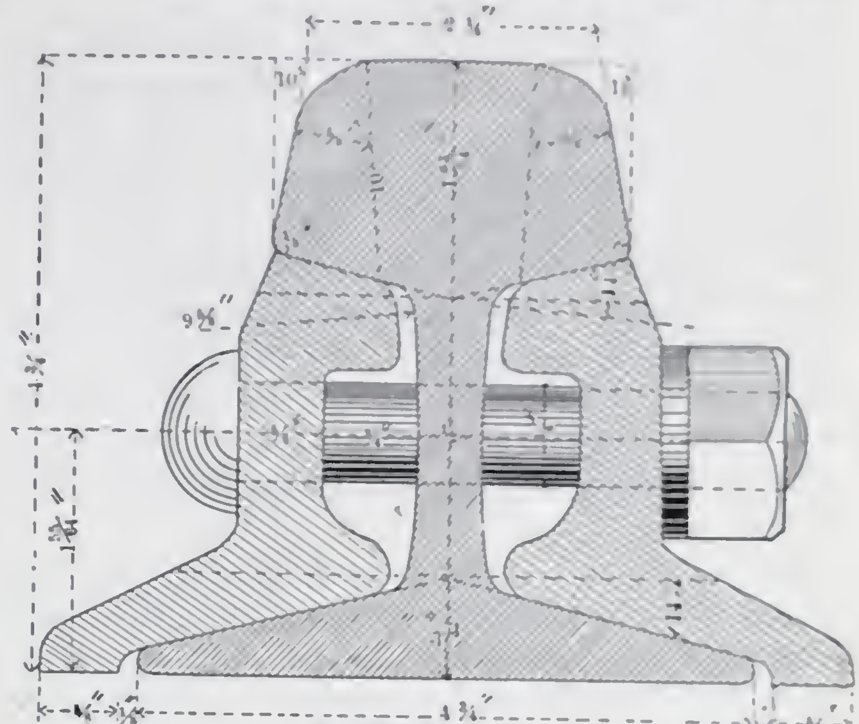
Rail 60 lbs. per yard.
Weight of Splice 36 lbs.

bolt to have projection to suit. I think that punching of material is a "relic of barberism" and think that with a little experience that the cost of drilling the holes would be but a slight increase over the cost of punching. Rail makers I feel sure would rather drill the rails than



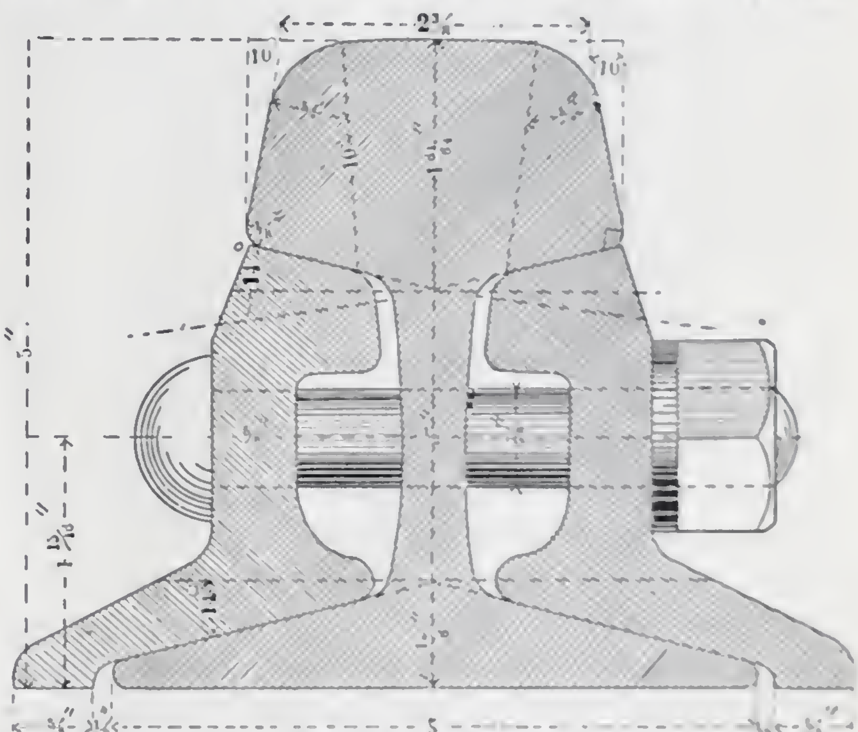
Rail 67 lbs. per yard.
Weight of Splice 40 lbs.

punch them, as the cost of maintaining drills, and drill presses, is far less than maintaining punches and punching machines. Mr. Sayre is also seriously considering the subject of cutting the ends of rails at an angle of 60°. This certainly will be a step nearer to a continuous track, as the space between end of rails can be reduced 30 per cent. while the crushing of joints could be partially guarded against. Mr. J. D. Hawlie, Chief Engineer of the Michigan Central Railroad, years ago had plans made to have the rail cut in the above manner and is one of the first engineers that discussed this plan. One thing is certain, to maintain a good roadway, particularly on the western roads using gravel ballast, better joint connections must be adopted than now ex-



Rail 76 lbs. per yard.
Weight of Splice 42 lbs.

ist, and I have no hesitation in saying that the majority of fish bar connections now in use are totally inadequate and inefficient, and from my personal observation of the roadway of the Lehigh Valley I think that the combination of rail



Rail 83 lbs. per yard.
Weight of Splice 45 lbs.

and fish bar connection is decidedly the best I have seen.

Mr. Darlington: I would like to ask if you adopt the method of the two round holes, it is proposed to have a man on each side of the bolt to screw the bolt up; also in making your bolt, how will you allow for expansion and contraction?

Mr. Jones: I would simply explain that ordinarily you drill a $\frac{7}{8}$" hole and you have $\frac{1}{8}$" expansion in the joint. You can readily make this bolt in the same way.

Mr. Darlington: What is to keep that bolt from turning? You say that a circular hole here (pointing to sketch) would do that!

Mr. Jones: That is illustrated by means of this drawing, his method.

Mr. Darlington: In other words, you expect to increase the cost about ten times of your splice bars in order to arrive at a result which you now have with oblong holes. Until the method of punching holes is given up the oblong hole is presumably the cheapest thing. As soon as you give that up then it would be time enough to talk about drilling those slot holes, but at present it would so far increase the cost of splice bars that it would make it out of the pockets of railroads, in the present depression of trade.

Mr. Jones: I will simply answer that by saying that your present fish bar is the most costly luxury you have.

Mr. Darlington: I deny that! You are just a year behind the present angle bar. The angle bar adopted by the Pennsylvania Company lines, and the Pan Handle lines, as the standard is not composed of pot metal and will stand a tensile strain which is equal to the emergency, and which is designed so as to give the greatest possible area as a bearing surface under the head of the rail. The fact which you have described of buckling out has never, in my experience, even with the old angle bar which broke so frequently, happened under any circumstances.

Mr. Jones: What is your bearing surface under the head?

Mr. Darlington: About $\frac{6}{10}$ of an inch.

Mr. Jones: We can easily prove that. You can not get that under the rail. Your rail is made in this form (sketch).

Mr. Darlington: I think, Mr. Jones, you are two years behind on your rail as you were one year behind on the splices.

Mr. Jones: I know exactly what the rails are, for I have made them.

Mr. Darlington: I do not dispute that, but our angle bar is as much superior to our old angle bar as our old angle bar was superior to the old fish plate without the angle.

Mr. Jones: In the first place if that is not composed of poor material punching may not hurt it, but if of poor material punching that sort and size of a hole in fish bars will weaken them, or else I do not know anything about engineering mechanics. Now as to the cost of making these fish bars and drilling that kind of a hole. To-day the question of drilling holes in steel rails and punching them is a question of economy. In the maintenance of machinery, I certainly prefer to drill holes to punching them. We have accomplished that much. While you may punch them quicker than you may drill, in the matter of cost you are way ahead on the drilling, I am satisfied that in designing work you can figure on slotting a little $\frac{1}{4}''$ or $\frac{5}{16}''$ hole just as cheap as any of these mills can roll and punch the angle bars.

A Member: I will endeavor to satisfy Mr. Jones that railroad car wheels should be tapered. The reason they are tapered is not because the molder wants to taper them. The wheels are tapered on account of the curves on the roads. I will just show you here (illustrating on board). If you have a curve the outside wheel must make a larger curve than the inside. Now, if you taper your wheel or both wheels, you get, as shown in sketch (illustrating on board) the circumference or rather the different radii that the wheel, which rests here on the rail, the radii of the curves the wheels will make, or, it will go through a longer distance if it rests near the inner end. Now, suppose the section of rail is like this (illustrating) on the curve, this being the outside of the rail and this the inside,—as soon as the car gets in the curve the outside wheel will be reversed, and the inside wheel will be like this, (illustrating) thus doing away with the tendency to press against the rail, causing the friction to be less. Now the engineers adopted it for this very reason.

Mr. Jones: I think I can convince you in a few moments you are wrong. In a taper tread wheel there is a difference in the periphery; now modern engineers in building curves elevate the outer rail and aim to keep wearing surface of rail in alignment. There is no reason why a taper tread wheel is necessary to run around a curve, and particularly so as the taper is in the wrong direction to do any good.

Mr. Darlington: I agree with Mr. Jones' remarks regarding the taper tread wheel. It is a relic of barbarism so far as practice is concerned. Mr. Hencke, in the first edition of his work, doubted the utility of it.

Mr. Jones: I forgot to mention one thing. Mr. Biddle (?) connected with the New York Central Railroad, in the Iron Age, gave a description of an 86 pound rail designed by the New York Central Railroad and in use in the Central Station and outer switch, New York. I thought I had that paper with me. The rail is made with a broad head and follows Mr. Sayres' outlines pretty thoroughly. He intended to follow the plan and have the rail with a large base (I should have said it has comparatively a thin head though broad) but owing to the objection of the rail makers he reduced the base, and in speaking of reducing the base of the rail to meet the requirements of the rail makers he said he added considerable to the stiffness of the rail, so loco-

motives in going over it, even with a heavy load, would distribute the load over two or three ties.

Suppose you take the old 65 pound rail of the Pennsylvania railroad, which no modern engineer would adopt to-day as a standard rail. Yet that rail was easier kept in line than the modern 67 pound rail. The roadmasters tell me that three men, with crow bars, could line the rail, but it is impossible to do that with the present section.

Take the Pennsylvania Railroad, the New York Central, the Pan-Handle, the Fort Wayne, it is simply nonsense to ask a 67-pound rail to stand the enormous traffic going over these rails. I do not think any of these lines should have less than 72 or 74 pound sections and the Pennsylvania should have nearer 80 pounds.

Mr. Henning: I have read Mr. Becker's paper on this question of calculating the strength of splice bars. He finds, under assumption of static strains, that the splice bar is too weak. I think the calculation by static strains is entirely out of the question. The proper calculation should be based upon the dynamic effect of a rolling load. It is all impact. The moment the load approaches a joint there will be a downward deflection, and the first effect is that the head of the rail strikes the top of the splice bar at the joint and compresses, or cuts into, it. It is a question of impact. Mr. Becker has shown that the static action of these excessive loads alone is injurious to the metal because he actually shows the strain existing there under that load approaches the elastic limit so closely that there must be a permanent set in the splice bars for these loads. Under the action of impact we know that the work done is twice what it is in the other instance, that it has twice the intensity of the effect produced by a static load.

Now, when we consider, as Mr. Jones has stated, the narrow support of the head on the splice plate, we readily see why they are broken on top. I think that if you pass along any railroad in the state, or out of it, you will find it as a fact that the splice bar, if a good one, will show a decided crushing effect on its upper edge after some service, and then a slight upward deflection in the bar will break it right there (illustrating). The top of it however will be so (illustrating.)

The rail which rests on here hammers in and gradually breaks the metal there, and the effect will be to crush the material before the strain has really been transmitted to the lower part of the splice plate. I believe Mr. Becker says he found a dynamic strain of 17,300 pounds. Now a load of 17,000 pounds on such material is sufficient to produce a permanent set with ordinary iron. Even if you take a very good iron it will produce a marked effect, a permanent deflection, or change of form at that point. When we consider all these things we see there is actually a weakness there, although we know that looking at it from a static point of view, supposing strains to be applied uniformly, that the plates may be strong enough theoretically; but theoretically does not mean practically because all our calculations are based upon the assumption that strains are applied uniformly and upon material which is entirely homogeneous and perfectly elastic. All railroad authorities know that. The whole plate does not take the strain uniformly. The top surface here must transmit all the strain to the other material below it, and a train going very rapidly will crush all this material and gradually break it off there, without transmitting the maximum effect throughout the section.

Another point is that the bearing the rail takes upon the splice bar is only about ½ inch, and if you will figure the pressure on that area, taking the load and distributing it over the bearing surface, you will find that the load is far beyond the elastic limit—the load per square inch, taking from the nearest tie to the rail joint, is beyond the allowable strain per square inch under compression.

In machinery we allow from 2,500 to 3,500 pounds per square inch on any surface where there is impact. In this case instead of 2,500 or 3,500 we actually have strains of 30,000 pounds. Now splice bars may be made of metal which may be anything at all picked out of the scrap heap. The railroad is perhaps represented by an inspector, a young graduate fresh from school, who knows nothing about iron and little about mills, and he perhaps is feted and taken carriage riding while the rolling is going on, and he does not know and cannot say what the splice bars are made of, and then the railroad company has to suffer for it.

I was merely going to call attention to the fact that I do not think it is correct to calculate the strength of the splice bar on a railroad track on the assumption that it is subjected to static loads. It is not a static load at all but a dynamic effect and should be calculated accordingly.

Mr. Becker: If the gentleman had read my paper more carefully, he would have discovered that I distinctly disclaimed the absolute correctness of the result of my theoretical calculations, which are based upon the assumption of certain quiescent static loads, and that I fully appreciate the dynamic effect of impact and vibration. But in the absence of experiments made under moving loads, which might afford a basis for calculation, I treated the question theoretically, assuming static loads, on the supposition that the ratio of results so obtained, of the strength of joint as compared with solid rail, would not

differ very materially from the ratio of strength under the effect of dynamic loads.

Mr. Henning: That is the very point I raised. There is no parallel between the two cases. In the one case we know what is going to happen but in the other we do not know. I think that although that analysis is perfectly correct if you assume a static load to be applied, it is not at all in accordance with fact when you render the effects as they are. It is well enough to make a calculation and think that the result ought to compare with actual practice, but we know that the assumption on which this calculation is based is not what you find in practice. I do not think the results in practice are what the results obtained might indicate.

Although we cannot calculate correctly the effect of impact we can approximate it sufficiently for practical purposes, and therefore should not judge of the strength of splice bars on the basis of a calculation based on the effect of static loads, as it will invariably lead to erroneous impressions and results.

Mr. Rodd: I was absent at the reading and former discussion of Mr. Becker's paper. He prefaced his remarks by saying the paper was of no great importance; it has however awakened a very lively interest not only here but wherever the technical journals reach. It is to such papers as Mr. Becker's and the thoughtful consideration which originates them that we owe advance in methods. A great point in such papers is that they confine the attention for the time to a given limited field.

In regard to the "Sayre" rails and joints Captain Jones has shown us, he has dwelt on the rail sections, which no doubt have much to do with the joint. Mr. Sayre is in the lead in this country in considering the joint question and in having adopted (doubtless after careful consideration of old methods here and in Europe) a joint of marked peculiarity and excellent efficiency, of which I have a sketch, see Fig. 1. The joint is not as originally designed quite like Captain Jones' drawing; it had the bolt swedged down to smaller diameter at center to allow for expansion without too large a hole in the rail.

This joint has the metal standing out under the side of head of rail. On the angle bar joints, where the rail is worn much, especially in severe side wear on curves, I have frequently seen the top of angle bar and even the bolt head cut away by wheels, as in Fig. 2.

If such heavy sections of rail as this (Sayre 83 lbs. rail) should come into use, their economy will depend largely on length of service. Such a head as this must give good wear to make it desirable; it must wear down so that the residual part must not be in too great proportion to that worn off; in addition, when nearly worn out, it must have the necessary strength and stiffness as well as when new. Take this rail, weighing say 125 to 130 tons per mile, you may wear 15 tons or more off the head, still 110 or 115 tons remain, or more than new 67 lbs. rail, although you may get 50 per cent. or more wear out of the heavy rail.

To come back to the joint, which was called the Fritz and Sayre, I would think it would be cut away on inner bar when rails are well worn,

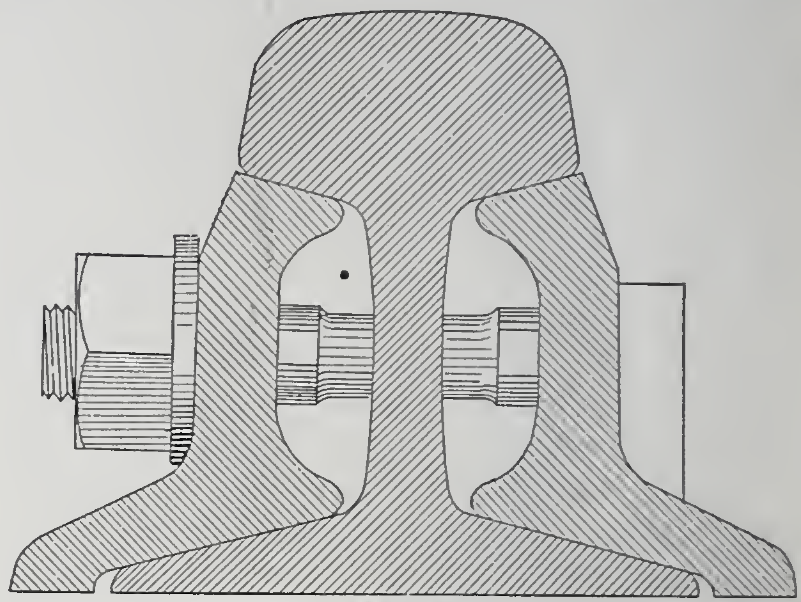


Fig.1

especially on lines with considerable curvature.

As to the wheel flanges and the shape of head of rail, that question is being ably discussed elsewhere. Captain Jones has confined his criticism to the weakness of the "Pennsylvania" joint and blames "the engineers" for not desiring a better section; I will only say that until more knowledge of facts is shown as to this, judgment should be withheld.

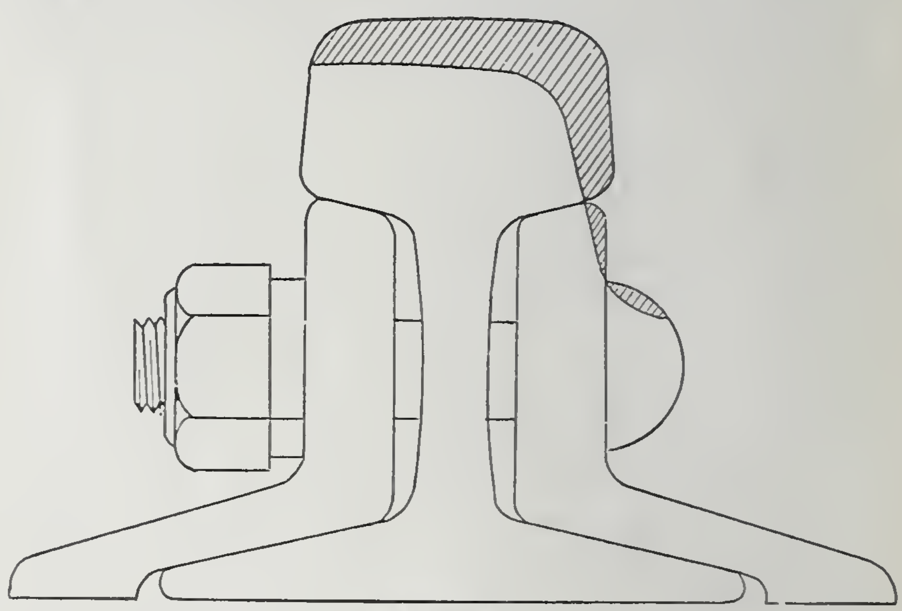


Fig.2

The punching of splice bars does not seem to me in any way objectionable; the bars do not fail at the holes and could only do so when very loose; punching is the cheapest way to make the holes and I see no force in the suggestion to drill these holes and then drill these smaller notches to keep bolts from turning.

Referring to the remark of Mr. Henning as to impact and wear, and failure of bars thereby, no

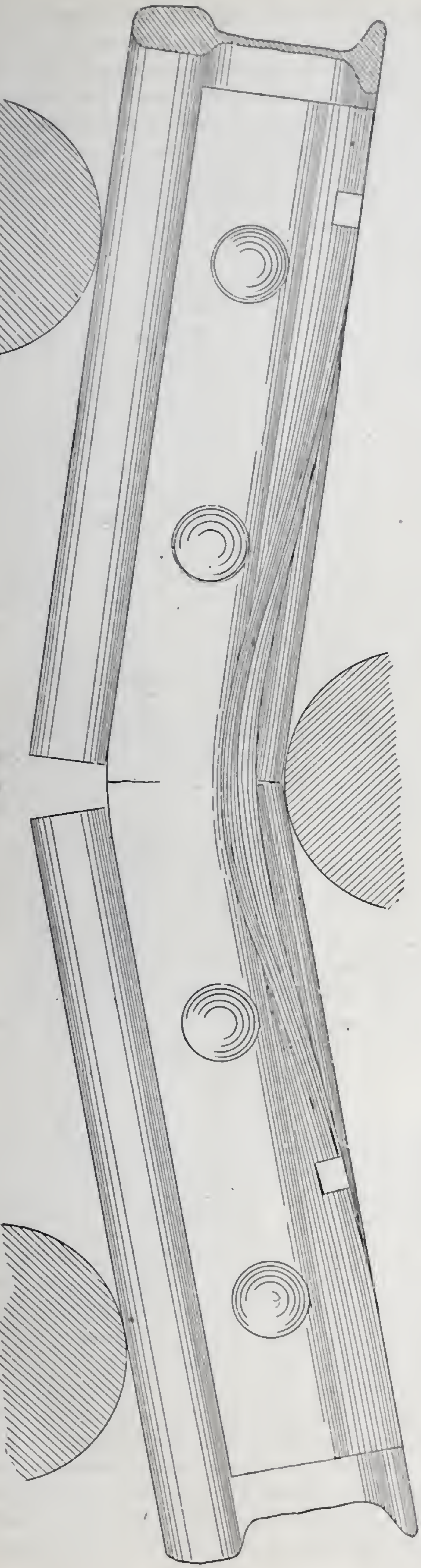


Fig. 3

doubt impact has much to do with joints breaking, but hardly in the way he mentioned. Such wear is only seen when the bars are loose. I have seen it in plain fish bars.

The tests quoted by Mr. Becker were made in

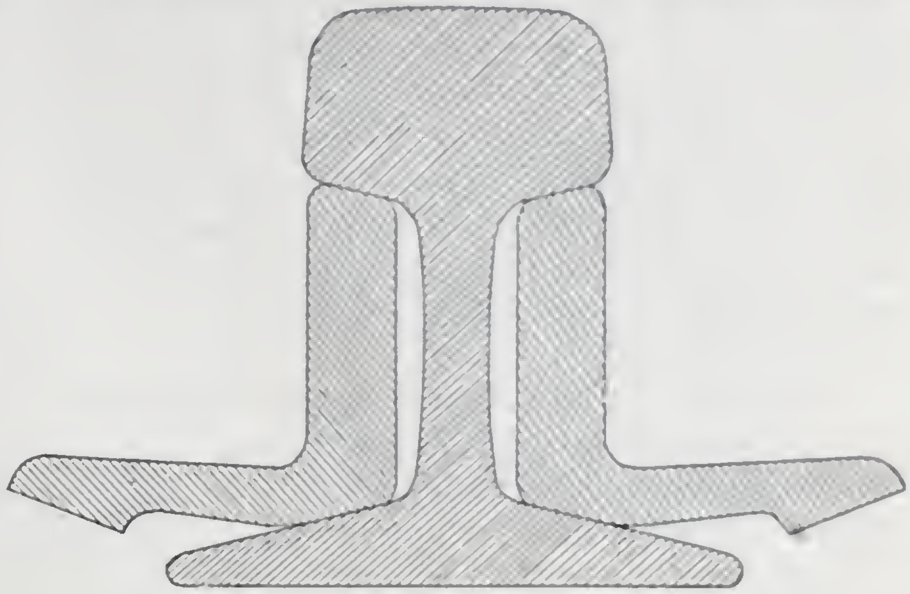


Fig.4

1876, by the American Railway Supply Company, to demonstrate the value of the Samson joint; I was present and took notes; they showed the Samson a strong joint so far as such tests go; they are only comparative; with one form they give a good comparison. Lately I have made a few tests, using the Pennsylvania Company splice. They are but single tests but fairly agree with

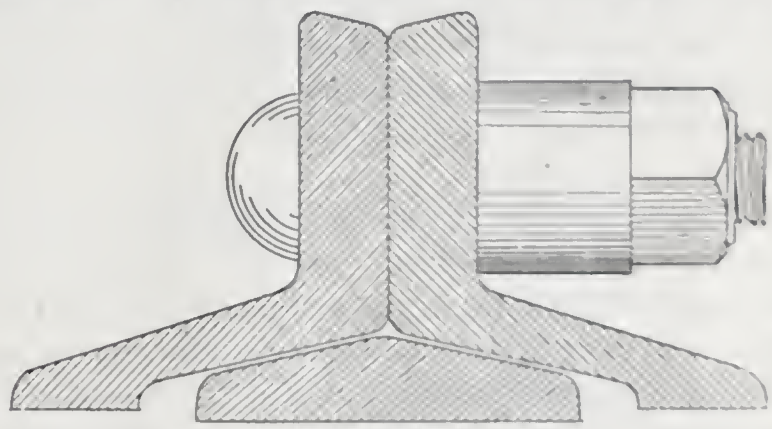


Fig.5

previous tests. All the tests were made by placing the pieces on bearings, 4½-inch iron cylinders, 20 inches apart, and applying force through another 4½-inch cylinder half way between bearings. The deflection and set were noted at dif-

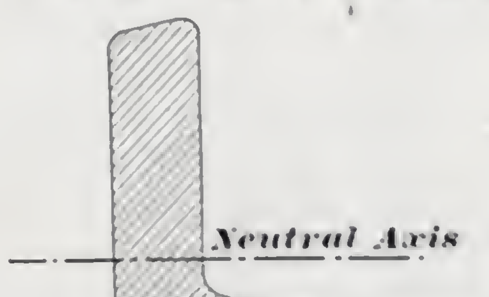


Fig.6

ferent strains but will not be quoted, as circumstances made it difficult to get correct measurements.

The samples were:

1. A 24-inch length of 67 lbs., 4½-inch steel rail.
2. A standard joint as above with two rail ends, each about 12 inches long, bolted up, with Verona nut locks; see Fig. 2 for section.
3. Same as No. 2, but test was made by placing head of rail on bearings and applying force at joint in base of rail; see Figs. 3 and 4.
4. Two angles bars bolted together as shown in Fig. 5, force applied at top; two small blocks were forged during tests to give bearing under bars; they were about 3 inches by 3 inches, of section shown, roughly fitting to bars.

The results were:

1. Showed a marked set at 100,000 lbs. strain. The greatest strain was 229,000 lbs.; rail was permanently bent down about 2½ inches from a line joining ends; no cracks.

2. Showed permanent set at about 32,000 lbs.; at 9,900 lbs., one end bolt was sheared off; this was ultimate strength; the bars did not crack; their flanges turned up as in Fig. 4; they were strongly gripped by the bolts to head and base of rail. This test agrees with American Railway Supply Company's tests, considering the different section; these bars would fit the (60 lb.) rail then in use.

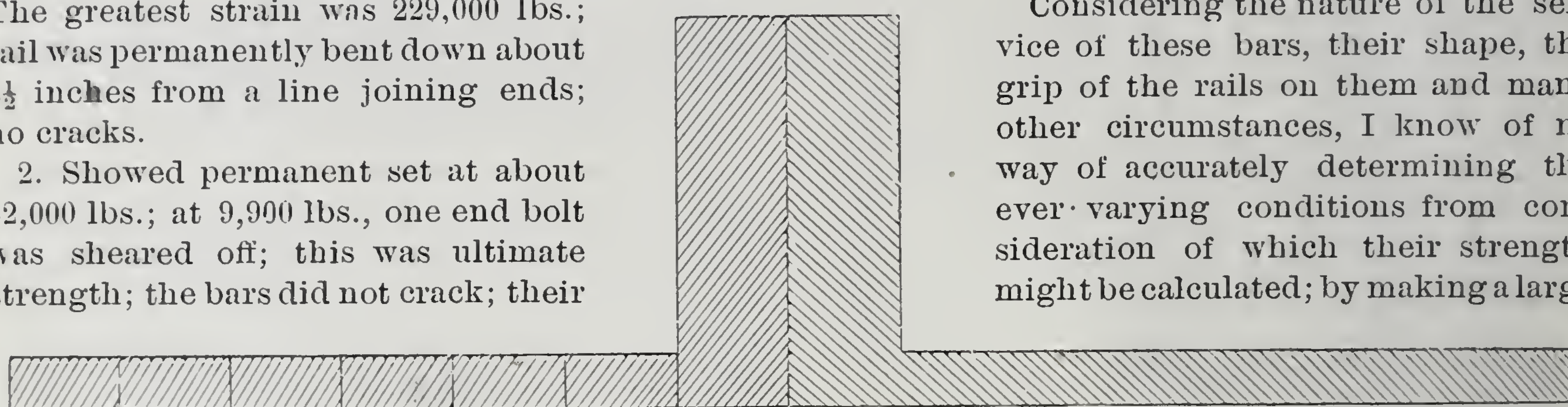
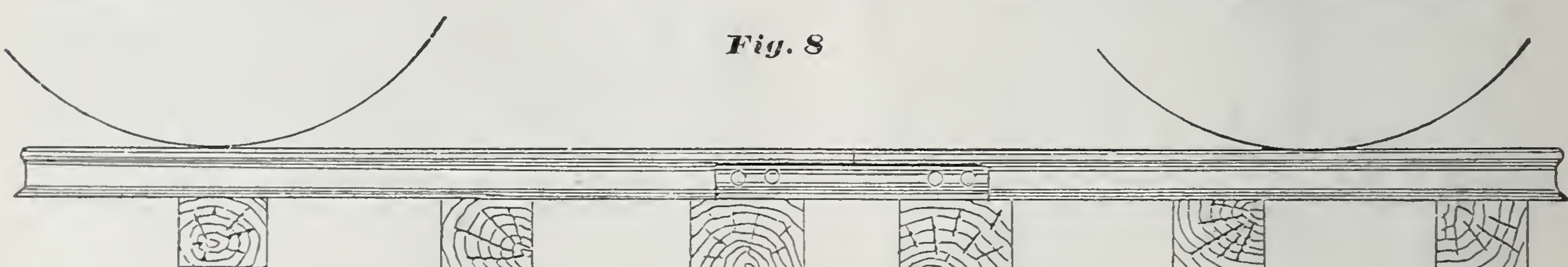


Fig. 7

3. Showed marked set (by the gauge) at 2,400 lbs.; the greatest strain was 48,000 lbs. This shows two-thirds the strength of No. 2 at elastic limit, which is the point to consider, and about half the ultimate strength of No. 2. The flanges turned up as shown in Fig. 4, as was the case with No. 2, although in this (No. 3) test the angles were bent in the opposite direction. One bar cracked as shown, precisely as the old pattern of angle bars cracked in service.

Fig. 8



4. The permanent set was marked at a strain of about 26,000 lbs.; the ultimate strength was 52,000 lbs. One bar cracked at angle, about 7 inches, in center in length of bar.

By comparing No. 2 with No. 4, we see that the joint strength is greater than the strength of the bars alone. No. 3 shows two-thirds of the downward strength for the upward strength. In No. 3 the rail was quite straight after the test; the main work of the bar to resist breaking was at the upper edge at the joint in rails; this is easily understood when we consider how the bars are gripped; the neutral axis in the bars alone passes through the lower part of vertical leg (Fig. 6), but the joint is, I suspect, considerably nearer the upper edge of bar, so that the resistance of those fibres has a smaller lever arm than in the bars alone.

There has been nothing said as to the service of the bolts; they affect the strength of joint decidedly, being screwed up tight and kept so. They are capable of sustaining 9,000 lbs. each in the direction of their length at the elastic limit.

Considering the nature of the service of these bars, their shape, the grip of the rails on them and many other circumstances, I know of no way of accurately determining the ever-varying conditions from consideration of which their strength might be calculated; by making a large number of experiments we should gain considerable knowledge. I have calculated the moment of inertia and the strength at the elastic limit in the usual way.

The two bars, as in Fig. 5, have a moment of inertia equal to 4.17 and would sustain, by calculation, 26,000 lbs. at 12-inch bearings at the elastic limit; this reduced to 20-inch bearings would be as 20 to 12, or 16,200 lbs.; test No. 4 made this strain at 20-inch bearing about 26,000 lbs. or 60 per cent. more than calculation, while test No. 2 would show the joint double the strength of the two bars as calculated. The calculation in the paper gives a strength of 17,300 lbs. at 14½ inches, which would correspond with 12,500 lbs. at 20-inch bearings, which is less than half the strength of two bars as in No. 4, and 30 per cent. less than by the ordinary calculation.

The static tests will not give us the strength of the joint, nor the calculations their value

In sections, as Fig. 7, the theory that the value of any particle's resistance is as the square of its distance from the neutral axis will not hold good. Considering a vertical force on the upright legs,

the value of the material in the horizontal leg probably decreases faster than its distance from the other leg increases. but with ordinary sections, if the lower leg is no longer than depth of other leg, the method generally used should give correct results up to the elastic limit. In an I

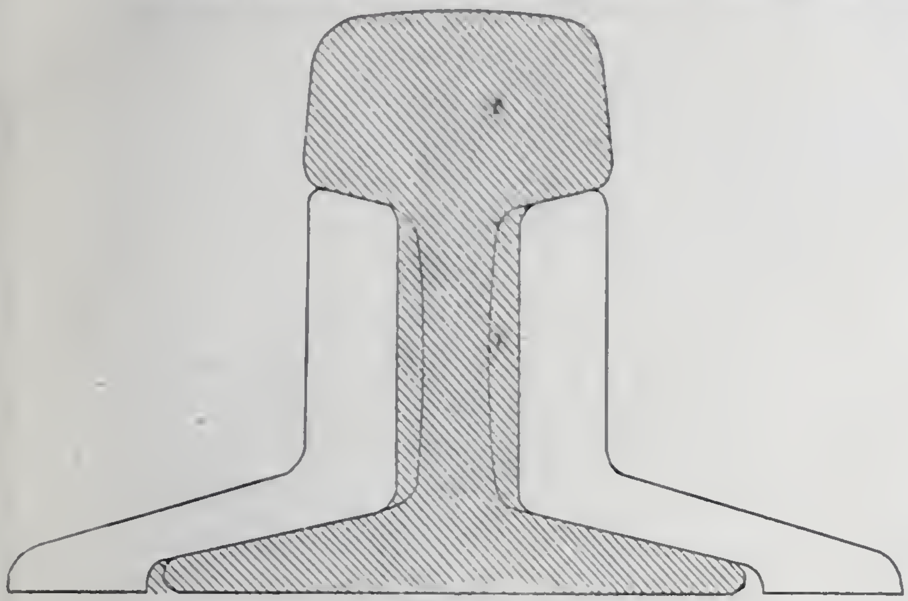


Fig.9

beam the outer flange fibres doubtless resist less than those at top and bottom of web, still the ordinary method of calculation gives safe and economical results.

In considering the strain on the joints, the case illustrated by Fig. 8 and similar ones, gives rise to severe work at joints and at the special place where the bars break. We have the joint ties of greater surface and supported on denser material, from frequent tamping, than the ordinary

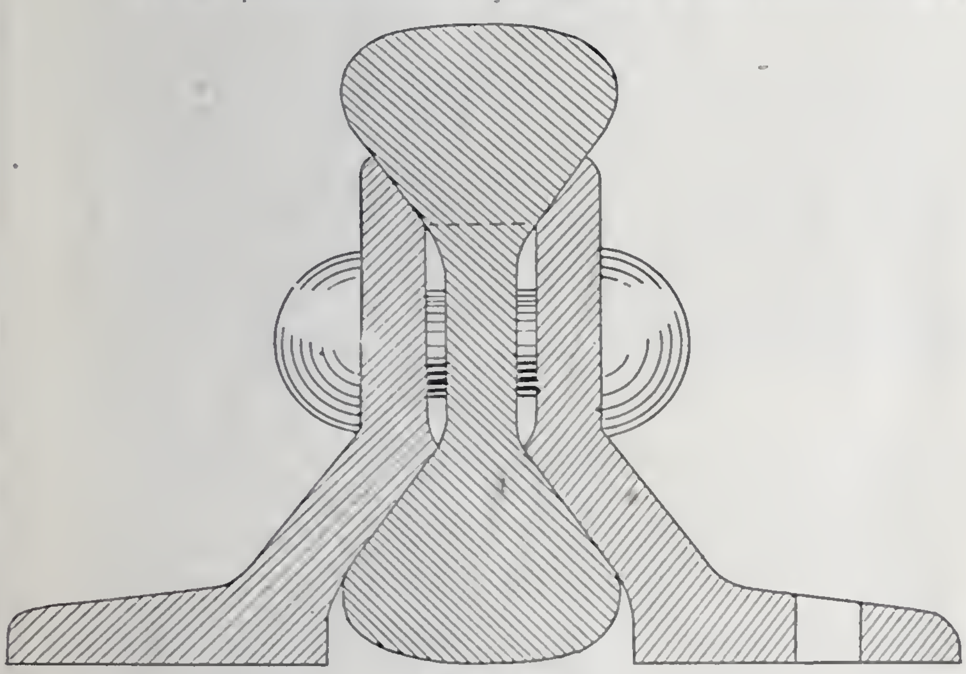


Fig.10

ties; under equal loads it may readily be that the rail over the ordinary ties may deflect several times as much as over the joint ties, making the rails levers over the joint ties fulcra, or placing the rail in the condition of a continuous girder, and in short giving the weak spot in the joint severe strain which is reversed as the wheels pass over it; it may sometime be that the joint ties are "down" for a while and then the downward deflection is large; by constant reversal of strain the old joint failed, sometimes when new and sometimes on bridges but generally after considerable service.

If we take the whole of Fig. 9 as a solid section, then for one-eighth or one-sixteenth of an inch in length we omit the shaded part, we have a condition somewhat analogous to that in our joints;

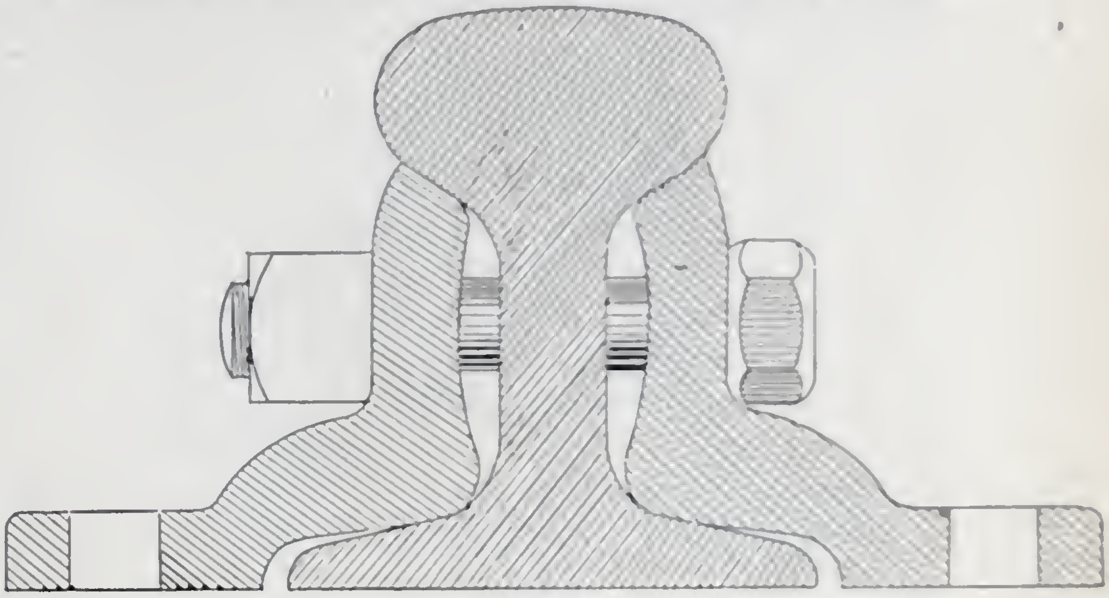


Fig.11

that the rail would strengthen the part unshaded could hardly be doubted; of course it is not in practice strengthened to such an extent.

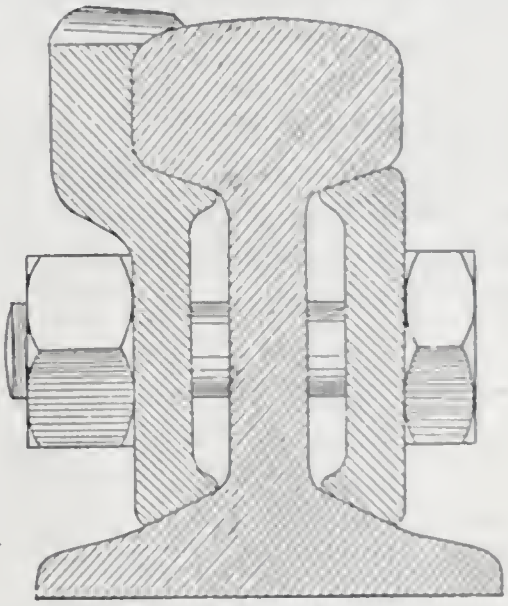


Fig.12

In some sections there is considerable metal over this outer part of rail flange, but it is of small value except to keep rails from creeping.

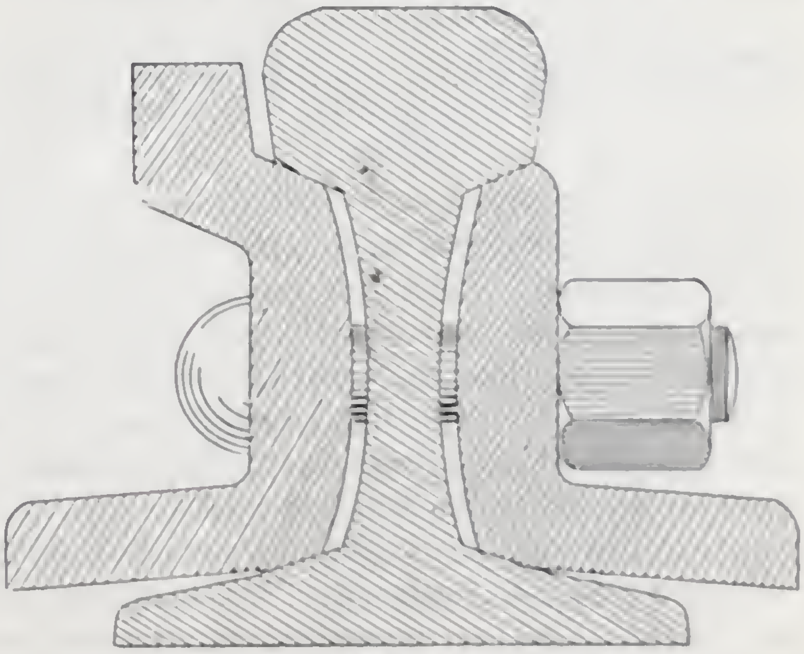


Fig.13

Years ago, with the chair iron, some rails would creep so as to make 6 inches in a day at a given point. The rails frequently crept unequally, sometimes in opposite directions. Many will call to mind pieces of track in which the ties stood some ten or fifteen degrees out of a square line to rails. This is largely remedied by the double angle

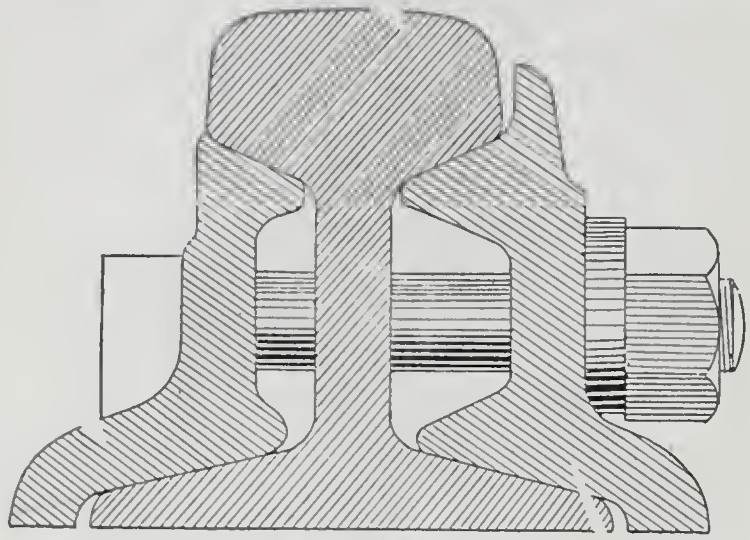


Fig. 14

joints. In the iron rails the flanges were slotted near the end.

Here are some sketches of old sections which I took occasion to look up; these and many more will be found in German and other technical publications.

The straight fish bar is said to have been inevnted by Engineer Trimble, of the Philadelphia, Wilkesbarre & Baltimore Railroad, in 1845. The earliest angle I have found, in Fig. 10, was designed in 1851 by Engineer Henz for the West-

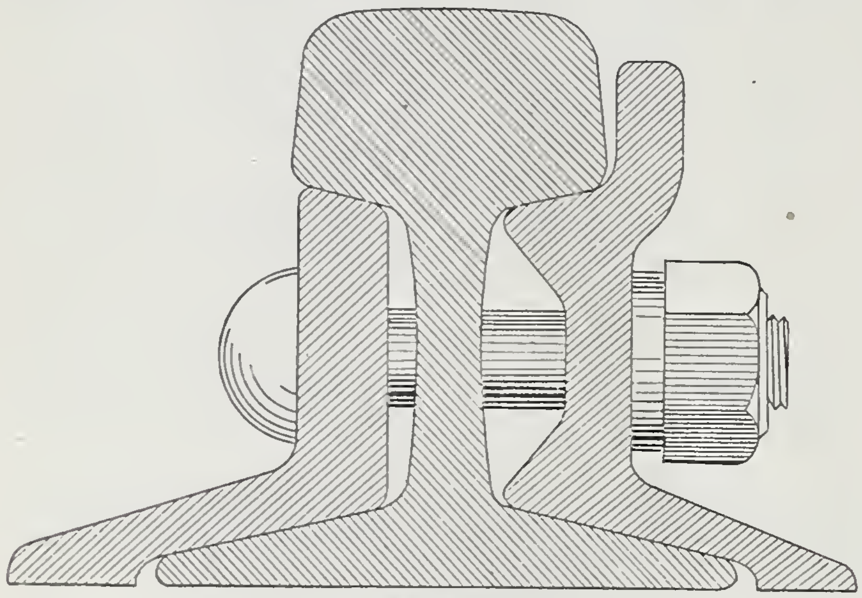


Fig. 15

phalia Railway for the double-headed rail. Fig. 11 was used on the Braunschweig State Railway. Fig. 12 shows the extension of the bar upward. alongside and partly over head of rail; it was designed to ease the wheel over the joint, though the extension over the head could hardly have been a practical feature; it was tried by Engineer Währer in 1870 with good results. (Von Waldegg's Handbuch, etc.) Fig. 13 was used on the Leipsic & Dresden Railroad. Fig. 14 is shown in the *Railroad Gazette*, April 1881, in an article by Mr. Sayre, which gives very much the same views on some points that Captain Jones has advanced this evening; the article says nothing, however, about this splice; it does mention a splice like Fig. 1 (sometimes called the Lehigh Valley splice). In Fig. 14, the extension upward terminates in a tolerably sharp edge so that the point of greatest fibre strain has a section easy to rup-

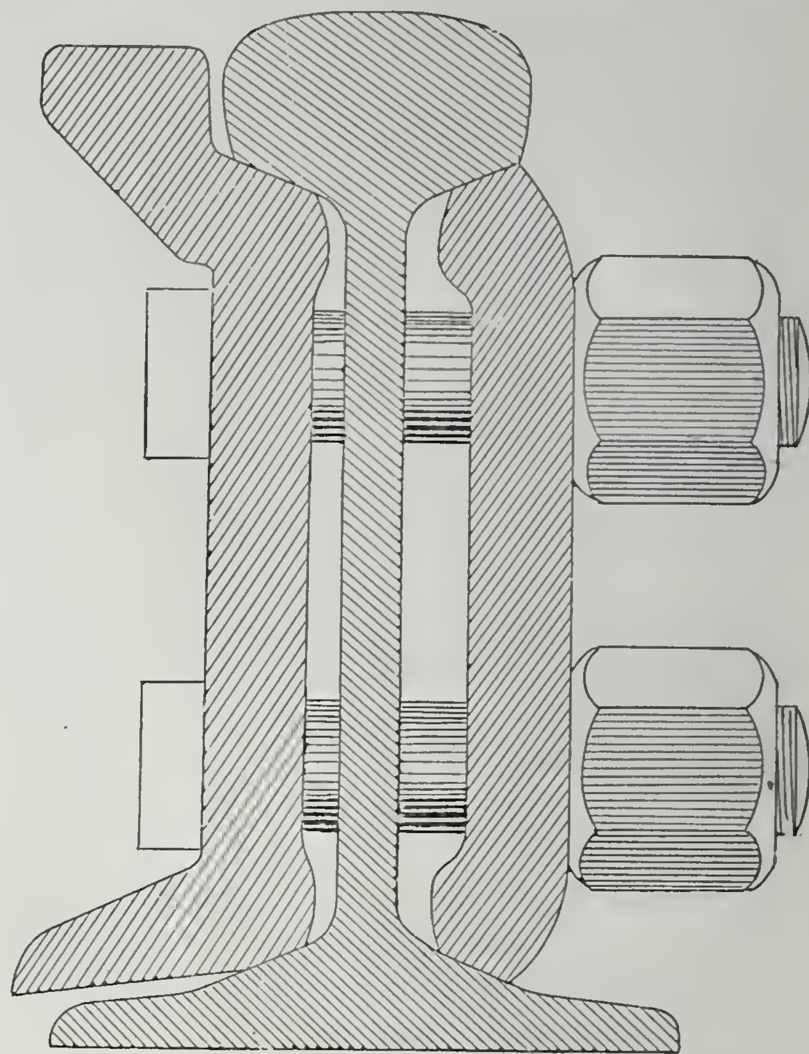


Fig. 16

ture; the placing of vertical part of angle so far from rail web is a point of doubtful advantage. Fig. 16 shows the "system Hartwich" in use before 1869 on the Cöln–Minden Railroad. Fig. 17 is part of the "system Hohenegger;" it was made symmetrical for upward and downward bending and is noticeable on that account.

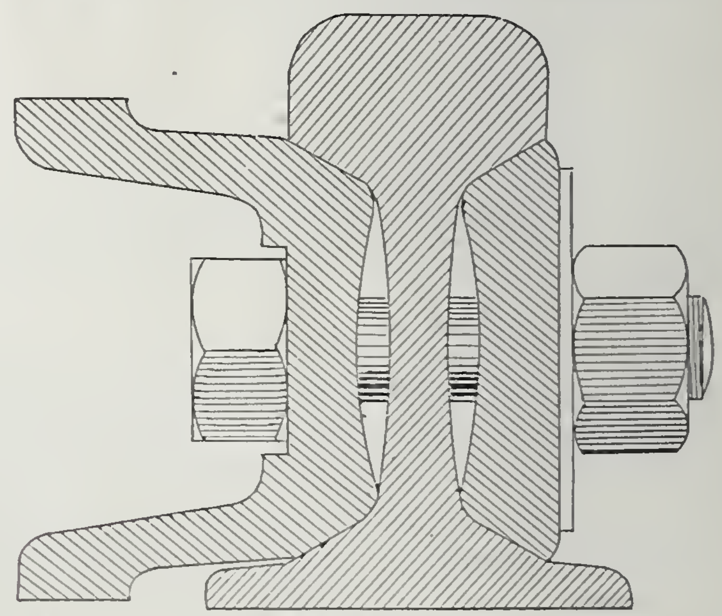


Fig. 17

Fig. 15 I have had worked up and believe it would make a good bar; it could go on but one side of rail, but would not make joint unsymmetrical enough to have a bad effect. Its moment of

inertia would equal 4.12, or almost as much as two bars such as shown on opposite side of rail; the neutral axis in this bar would be above the center of bolthole. The weight would be about the same as the bar shown on opposite side of rail. No doubt it would be a little more expensive as two sets of rolls would be required. All the bearing surface needed is obtained and we thin down here and there (indicating) at line of bolt holes and over edge of flange of rail, and get metal where most needed. The bar shown on opposite side of the rail may be cut down like the other over flange of rail. The weight of a pair would be about 35 lbs.

One other matter I would like to mention though it is rather late. In walking over track I have often noticed a wavy appearance of top of rails, the crests and depressions very gently rounded, perhaps from eight to twelve inches apart; the variation from a straight line is hardly remarkable, one or two hundredths of an inch or so; it occurs more at the ends of rails but sometimes at other points and is easily detected by alternate bright and dull surface. I have not in my mind accounted for this by any work in the mill, though it may be due to this, but rather to alternations of sudden increase and decrease of weight transmitted through wheels,—the impact from this and from drivers is an actual condition constantly recurring. A wheel while traversing a rail may and does transmit greatly differing weights at different points. This I believe has something to do with this wavy appearance. If this action does not originate the waves then the waves contribute to this action. Take it either way this causes wear of rails and hard service of joints. I have lacked opportunity to make measurements or have photographs made of this peculiar appearance; doubtless many present recognize the description and may be able to account for it.

MR. JONES: It is possible that has been partly done in the mill. All rails rolled should show by the head that the rolls were filled in the last pass, which would be equivalent to drawing through a die. Inspectors generally object to our filling the heads, but for a good surface on the head of the rail, I would always prefer to slightly overfill; in other words, to show on the rail the dressing line of the roll.

MR. RODD: This sketch (illustrating) is very much exaggerated but it is something like this. These spots here would be bright while those would be dark. You may think it would be due to some peculiarity in the metal, but I do not think so.

MR. DARLINGTON: A good deal of that darkness is caused by the rails being bent in loading and unloading. I have noticed very many times with our rails, where rails were slightly bent, that a bright spot would be the result, with a depression following beyond.

MR. RODD: I merely say I know how much straightening is done on the track and there is not enough to account for it in that way at all.

MR. DAVISON: I have noticed in counting rails before they were used that same appearance, and after use, the wheels passing over them would increase that wavy condition.

MR. RODD: As to the material in splices, I think Captain Jones is possibly mistaken in condemning it in the terms he has just used. A few months ago I was making calculations of rail and joint sections—some modifications here under discussion. I took the opportunity to express some strong views in regard to the "poor material," just as the Captain did, and believed it to be a poor soft *dead* iron, the cheapest material that could be made to take the required shape; at the same time I had never made nor heard of tests to determine the quality and so wrote to have four or five bars sent at random from any on hand along the tracks. These bars were cut into strips (avoiding the bolt holes a little) and tested in tension and by bending hot and cold. The result was a complete surprise. The iron by the records stood from 51,000 to 56,000 pounds per square inch and averaged, in the six tests made, 53,000 pounds. Two pieces here (illustrating) in lower part of bar stood 51,000. The elastic limit was from 28,600 pounds to 30,600 pounds per square inch, fairly uniform, averaging 29,700 pounds. The stretch in 8 inches was good, averaging 15.2 per cent. The bending tests as would be expected from the above were very good; in fact the iron would pass as first-class bridge iron. I am far from saying that these tests show the character of iron generally used for splice bars, but as to quality we should have good evidence and not condemn it wholesale.

MR. STROBEL: I wish to say only a few words, as the hour is late. The common angle bar splice was certainly not based on correct static calculations. These are fully adequate to establish its inefficiency. I am sorry Mr. Becker did not push his investigations further, and calculate the upward bending moment occurring at the joint by proper loading of the spaces adjacent to the joint space. He would have found that the upward bending is very considerable; that if you assume the joint ties as tamped slightly higher than the adjoining ties—which is the condition very frequently found in tracks that are well kept up, especially before inspection—then the upward bending moment at the joint may be greater than the downward bending moment. The position of the wheels shown in Fig. 5 is not the po-

sition which would give the maximum upward moment. The wheels should be distant about one-third of the space from the joint tie. Mr. Rodd's tests show that the angle bar joint is only about one-half as strong bending upward as it is bending downward. The calculation would show the same result, due to the fact that the center of gravity, and consequently the neutral axis, is very low, being located near the horizontal flange of the angle. Now, the joint is weak considered as bending downward, since it is only about half the strength of the rail. It is however much weaker considered as bending upward, and therefore it is not surprising that the joints break in the way described by Mr. Becker, namely; by fracture in the top. It is also not surprising that the joints give way most frequently when they are new, and especially in good tracks, as on bridges. If the splice bars do not fit well and if the nuts are loose, the bars act simply as liners, and will not resist the vertical forces. The splice bar joint is then no better than a plate joint. In old tracks you will probably find very few joints which are efficient.

By continuing the investigation, the dynamic forces mentioned by Mr. Henuing would have to be taken into consideration. It will be found for instance, that a very powerful centrifugal force is created by the downward deflections of the rail between points of support, equal to as much as 25 per cent. of the load for a very swiftly moving train. The impact then would add, say, another 50 per cent. to the strains obtained for static conditions.

The Sayre joint is, I think, a move in the right direction. Material is added to the top of the splice bar where it is most needed. I still think, however, that the Sayre joint is weaker than the rail; but it is a question whether it is necessary to make the joint as strong as the rail.

The calculations I refer to assume that the joint produces continuity in the rail. The ideal joint is one which will produce perfect continuity, and this the splice bar joint attempts to secure. It is therefore necessary to apply the theory of the continuous girder to obtain the moments and strains, and the results are as stated.

Mr. Rodd: Mr. Strobel is right I think in speaking of continuity—that is the word. If we can restore the continuity of the rail we have what is wanted. The bars make the rail much stiffer at the ends; then they are strained at our weak point by the effort (to make the bend) a continuous girder takes over an unloaded span (between joint ties) when there are loads on adjoining spans as in Fig. 8 or similar dispositions of loading.

In regard to strength for upward bending I should say it was about two-thirds of the downward strength as that, by the test, was the proportion at the elastic limit.

The elastic limit — that effects our calculations. If you strain a piece of metal to the point where further straining would produce a permanent set we generally say it is not injured. But this permanent set is produced by a very slight addition or by any addition to load. Is it this addition which injures the piece? I think not but rather the load already imposed has strained some of the fibres beyond the elastic point and the inertia of the piece alone has restored it to its normal condition so far as distortion is concerned. At the point referred to there is a nice balance but any addition to load gives a set. This may account for discrepancy between our calculations and the tests; at the calculated strength the bars might not suffer, but before the bars received their set under test they may have sustained injury.

In regard to steel, I intended to mention it before; I believe it would make a good splice bar and have no doubt it will be tried carefully.

Mr. Zimmerman: Mr. Strobel and Mr. Rodd have touched upon the upward movement. It seems to me that movement is not only greater but occurs twice where the downward only occurs once.

Mr. Jones: I would just say that on the Pennsylvania road the very best steel rails will not wear over six or seven years on their curves.

Mr. Taylor: With regard to steel splice bars, Mr. Jones, do you know of any in actual use?

Mr. Jones: I do not know of any. It is claimed that they wear the rails.

A Member: It seems to me that is something we should know about. I know that iron wears them.

THE TORSION BALANCE.

[A paper read before the Engineers' Society of Western Pennsylvania, April 21, 1885, by WM. F. ZIMMERMANN, M. E.]

I have the pleasure to-night of exhibiting to you two samples of the Torsion Balance, the invention of Frederick A. Roeder, recently deceased, and Dr. Alfred Springer, of Cincinnati. They are weighing machines, in which knife edges are dispensed with, and replaced by flat steel or other metal wires or bands which are subjected to slight torsion.

Weighing machines which depend upon knife edges are at the best, very imperfect pieces of apparatus. When the knife edge is in its most perfect condition it is still not without friction which impairs the sensitiveness of the balance; and the more perfect the knife edge is the more easily it is made imperfect by the friction consequent upon its use, by dust, by corrosion, or by flattening or bruising through over weight or repeated use. The balance is apt to get out of order by shifting of the knife edges and bearings and it is notorious that the errors of weighing in railroads, manufacturing and commercial establishments are continually causing serious loss to buyer, seller and transporter of merchandise. In professional work, such as that of a chemist, whose accuracy depends upon that of his balance, the imperfectness of knife edges and their liability to get out of order is a constant cause of annoyance and expense.

The Torsion Balance overcomes all the objections to the knife edge. In the words of Mr. James W. See, of Hamilton, Ohio, the well-known "Chordal" "it gives us everything we desire and nothing we object to; a scale joint in which one piece does not rub upon another; a scale joint in which there is no friction; a scale joint which cannot possibly shift its position; a scale joint unaffected by shock; a scale joint unaffected by dust, or rust or acidulous vapors, or acids themselves; a scale joint not injured by overloading; a scale joint in which there is absolutely no wear; a scale joint which permits of the construction of a scale upon the usual principle of simple and compound levers, the entire structure being practically in a single piece."

In its simplest form the Torsion Balance consists simply of a wire or band placed horizontally and tightly stretched between two supports, and to the middle of this band is soldered or otherwise firmly attached, at right angles, a beam at its middle point. If equal weights are suspended, say by silk threads, from the ends of the beam they will balance each other, and the beam will remain horizontal, but if a very slight overweight be placed at one end of the beam it will tend to make that end of the beam go down, and as it goes down the supporting wire will be slightly twisted, hence the name the "Torsion Balance." This principle of weighing has been known for a hundred years or more, but until recently it has always failed in practice, for the reason that if the supporting wire be made large enough to sustain any appreciable load, the stiffness of the wire or its resistance to torsion was so great that the balance ceased to be sensitive; it took too great an overweight to cause one end of the beam to go down. This hitherto fatal defect of torsion balances has been entirely overcome by Messrs. Roeder & Springer. They use torsion bands or wires which are strong enough to sustain much more than the maximum loads to be weighed and they increase the stiffness and resistance to torsion by strongly tensioning them over firm supports; but they absolutely counterbalance the resistance to torsion by the use of another force which acts in opposition, viz., the force of gravity. They do this by making the center of gravity higher than the point of rotation, and the simplest means of doing this is a heavy ball or poise, placed immediately above the point of rotation. If the torsion band had no resistance this poise would cause the balance to be top heavy, or in unstable equilibrium. The weight tends to fall on one side or the other; its tendency increasing as the sine of the arc of rotation. By adjusting the weight vertically its tendency to fall may be made exactly the same as the resistance to torsion of the band, which resistance increases directly as the arc of the rotation. As for small arcs of rotation the sine is the same as the arc, and the forces balance each other, the balance thus becomes infinitely sensitive, and the smallest possible overweight will cause

one end of the beam to descend; if the weight is too high it will be top heavy; if too low, it merely decreases the sensitiveness of the scale, and this sensitiveness may be regulated to any degree desirable by adjusting the position of the weight. For ordinary grocers' scales too great sensitiveness is not desirable, and the weight is put some distance below the neutral point, but in assay balances the weight may easily be adjusted to such a point that the balance will indicate with ease a weight as small as one two-hundredth of a milligramme. Such a balance is now in possession of Dr. Springer, and it is believed to be by far the most accurate balance in the world, and at the same time it is far more durable, more easily handled, and less apt to get out of order than any other assay balance in existence.

During the last three years Messrs. Roeder & Springer have been quietly at work in a small room in Cincinnati, making a complete line of the scales and balances, including the assay balance above mentioned, a large analytical balance sensitive to one-fiftieth of a milligramme, several drug and prescription scales, grocers' and candy scales; a meat scale made by replacing the knife edges of the Buffalo Meat Scale with torsion bands, and a 1,000-lb. platform scale. The scales we have with us to-night are a Druggist's Prescription Scale and a 5-lb. Candy Scale. As in these scales the pans are above the beam it is necessary to have two beams, the lower one taking the place of what is known as the radius arm in ordinary counter scales for keeping the pans vertical. You will notice that torsion bands are at the ends of the beams as well as in the middle, so that knife edges and all rubbing parts are entirely dispensed with. In the Candy Scale each of the beams are made double, but this is not at all necessary except as a matter of design, as much larger scales are made with single beams. By taking off the poise of the Candy Scale you will notice that its sensitiveness is entirely destroyed, but by placing it on and adjusting it to the proper height and turning the heavy side of the ball into such position that it neutralizes the error in level of the table upon which the balance is set it may easily be made sensitive to the 100th part of an ounce. This I think you will admit is sensitive enough for a Candy Scale. You can weigh letters on it to determine how much postage they need with far greater accuracy than with any letter scale now in the market.

The Prescription Scale has an arrest attached to the pans operated by a lever outside of the case; it is a very ingenious device for the purpose.

The torsion bands in these scales are made of spring steel, brazed together, and stretched over the cast iron frame by means of small wedges until they are tuned to a certain note. The three wires may be fastened to the beams so that the end ones are absolutely equidistant from the center one by cutting the grooves in the beams with proper machinery; thus for all scales of this class absolutely no hand adjustment of lever distance will be necessary. You can see, therefore, that the scales can be manufactured cheaply; that they will be durable is almost self-evident. With the very slight amount of torsion which is given to the bands at each oscillation there is no reason to believe that they will not last one hundred years. Corrosion, unless it eats through the band will not impair the accuracy of the scale; the bands are strong enough to sustain more than double the maximum weight for which the scale is sold without being strained to their elastic limit.

The complete line of scales which Dr. Springer has recently exhibited in New York show a vast variety of detail of construction and artistic design which it is not my purpose to enter into. I merely bring these scales before you to show the general principles involved and I think you will agree with me that they promise a very important revolution in weighing machinery.

DISCUSSION.

MR. ZIMMERMANN:—It may be difficult to discuss the question without looking at the scales. If any of the members choose to look at them and ask questions about them, I think you may accord them that privilege.

The principle is something that may be applied to any size scale. Of late I have been seeing if it could not be applied to a testing machine. We know the knife edge is done away with in Mr. Emery's testing machine, and I think this principle of torsion can be applied to the same purpose, although it has not yet been carried far enough to determine.

BY A MEMBER:—I would like to ask in regard to one point the gentleman spoke of, in regard to that ball on the Candy Scale, which is slightly eccentric, and which is intended to adjust the scale in case the base is not quite level, or its plane not quite level to the plane of the torsion band. In the first place, then, if you turn that ball so as to throw the balance a little to one side or the other, do I understand aright, is the band al-

ready under something of a twist, due to that correction?

MR. ZIMMERMANN:—It will be under a slight torsion.

BY THE MEMBER:—The question then is whether that has interfered with or changed the degree of sensitiveness of the balance—whether starting from that condition of the band you have the same sensitiveness as if the base had been perfectly level in the first place, or its plane level to the plane of the torsion band to begin with, and if that adjustment affects the sensitiveness, if carried to extremes?

MR. ZIMMERMANN:—It might, if carried to extremes, although I have not noticed it in the experiments I have made with this balance.

That balance (referring to the Candy scale) is far more sensitive than there is any need for. A balance of that sort should never be more accurate than one-eighth of an ounce, for ordinary candy weighing. At the same time the principle is such that it can be made almost infinitively sensitive.

I do not think that the slight torsion that might be made through one end being a little higher than the other would affect its sensitiveness, at least so little as not to make any difference.

I might have said in my paper that the prescription balance, is leveled by means of thumb screws on the base board. It has the same adjustment that an ordinary chemist's balance has, so that you get it perfectly level, the ball, in the case of the prescription balance or chemical balance being concentric, and the adjustment is made with thumb screws on the side to level the base, the same as you would in an ordinary chemist's balance.

MR. PHILLIPS:—Are the very delicate balances you spoke of being made and used?

MR. ZIMMERMANN:—I believe there is one being used in Cincinnati, that was the gift of Mr. Springer. He has one in New York on exhibition, which was examined by the Yale & Towne Mfg. Co. and by several of the prominent engineers of New York, and was found, I believe, to be sensitive to the one hundreth part of a milligramme.

Of course, in scales of that sort, the correctness and accuracy depend on the beams being equidistant from the center. The scales weigh—in fact all the small scales and the larger ones too—with poises, the pan or side in which the material is weighed and the pan for applying weights being equi-distant from the center.

The chemist's scales are made exactly the same way. They rely entirely upon putting weights on to weigh, and they can be made as accurate as machinery, or hand, can possibly make them.

In the finest scales, I believe, the bands are made of the very finest watch spring wire.

STANDARD RAIL SECTIONS AND FISH BAR JOINTS.

[A paper read before the Engineers' Society of Western Pennsylvania, by WM. R. JONES, General Sup't. of the Edgar Thomson Steel Works.]

Year after year adds to the vast number of rail sections designed by Engineers, and Engineers differing as to what is, or should be standard sections. A few months ago I submitted drawings of the Robert Sayre Standard Sections as designed by him.

I find a large number of Engineers who will not agree with Mr. Sayre on this subject and are opposed to the standards he has suggested, as the great evil of useless multiplicity of sections still continues, greatly increasing cost of rails per ton and often involving rough rails, owing to excessive cost of maintaining duplicate rolls and the great difficulty of securing yard room to store the rolls.

I have finally mustered sufficient courage to present, through the society to the Engineers, a series of Standard sections which shall range from 50 lbs. to 78 lbs. per yard.

One of the first objects I have held in view is to get a comparative equal distribution of the metal in the sections. I think our railroad engineers are subject to criticism in this, that the major portion aim to mass the metal in the head of the rail, as they think to obtain better service and longer life of rail, thereby sacrificing strength. I think it will be safe to assume in the start, that one important feature of the rail to consider would be its strength. If we start with massive heads, thin web and thin flange, we at once find the neutral axis too near the top for safety, while the web and flange are so dense, on account of a lack of metal and by reason of cold rolling, that the web and flange have comparatively little ductility or elongation, while the head will show good in both ductility and elongation. I think a very important feature of the rail should be fairly considered, viz: its tensile strength in the direction of strains tending to break the rail, and I think it is fair to assume, in the majority of cases, the flange must part first, before the rail can break.

Now if this assumption is correct, then why not distribute the metal so as to get as far as possible great tensile strength in flange, consistent with good wear and good service of the rail? Again, if we consider the great liability of the head and flange of the rail to be alternately subjected to compressive and tensile strain, owing to changeable condition of road bed, then the necessity for a more equal distribution of metal becomes more apparent.

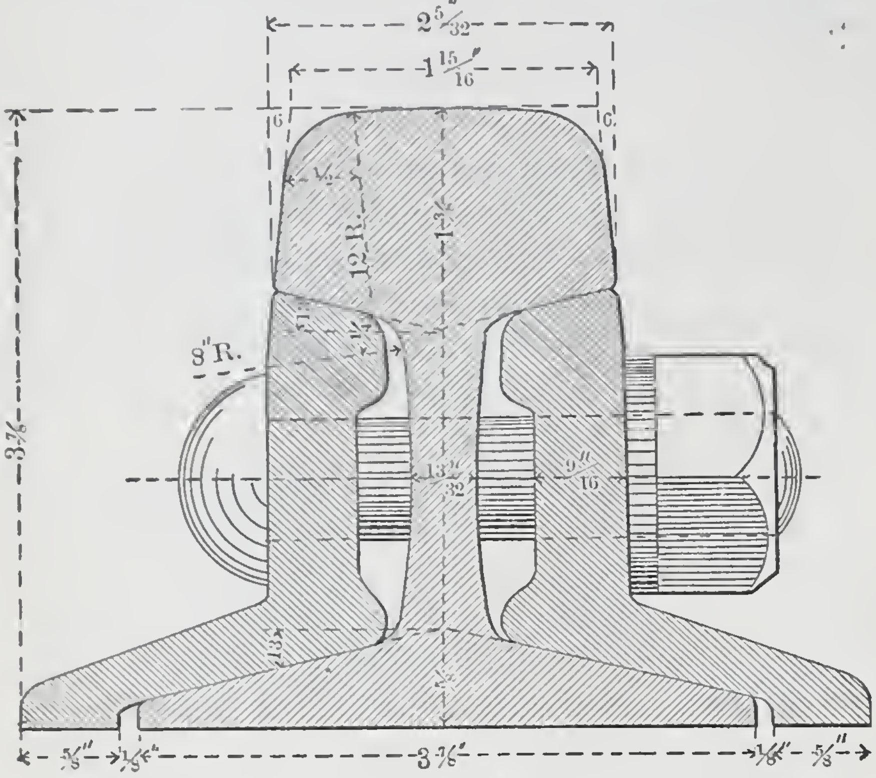
I believe that it is a great mistake to put too much material in the head of the rail. I am certain that the metal is not so compact nor so dense in a section that has, what I consider, an excess of metal in the head of the rail, and, if the rail makers could only get railroad Engineers to adopt some Standard sections, in which the metal is more impartially distributed, I am confident that far better results will be obtained, both in the safety and wear of the rail. I do know that it is an absolute fact that in rolling rails of improper design, that great strains are imbedded in rails, owing to the unequal distribution of metal, and the contortions the rails are subjected to in cooling.

In the section I now submit to the society, I start off with fixed angles and radii, up to the 70 lb. section, where I increase the radii, connecting head with web and web with flange; this is done to better support the head. The radii from top of rail to side of head is ½ inch in all the sections. This I adopted to meet the views of the master car builders. The angle of side of head is uniformly 6 degrees. The angle under the head and the angle of the flanges are 13 degrees; the radii of the web are 8 inches.

You will notice that the edge of every flange is comparatively square, a slight radius on upper edge of the flange and the lower corner slightly rounded to prevent cutting into the ties. I have made the flange thus, so as to meet a very important objectionable feature of the rounded edge flange, viz., the cutting or the wearing away of the spikes. I am reliably informed by one of the best road masters in the country, that the wear and destruction of the spikes by the modern, thin, rounded flange, is fully 60 per cent. greater than by the old style of flange. So

far as is posssble these new sections meet that difficulty.

Another feature in the construction of the rail, which is similar to the Sayre section, is the small radii connecting the side and under side of the head of the rail; this is done to secure good contact surface for fish bar. No rail section that I have ever seen, aside from the Sayre section, gives, in my opinion, enough contact surface for fish bar, as the joint is the weakest part of the



Rail, 50 Lbs. per Yd. Weight of Splice, 26 Lbs.

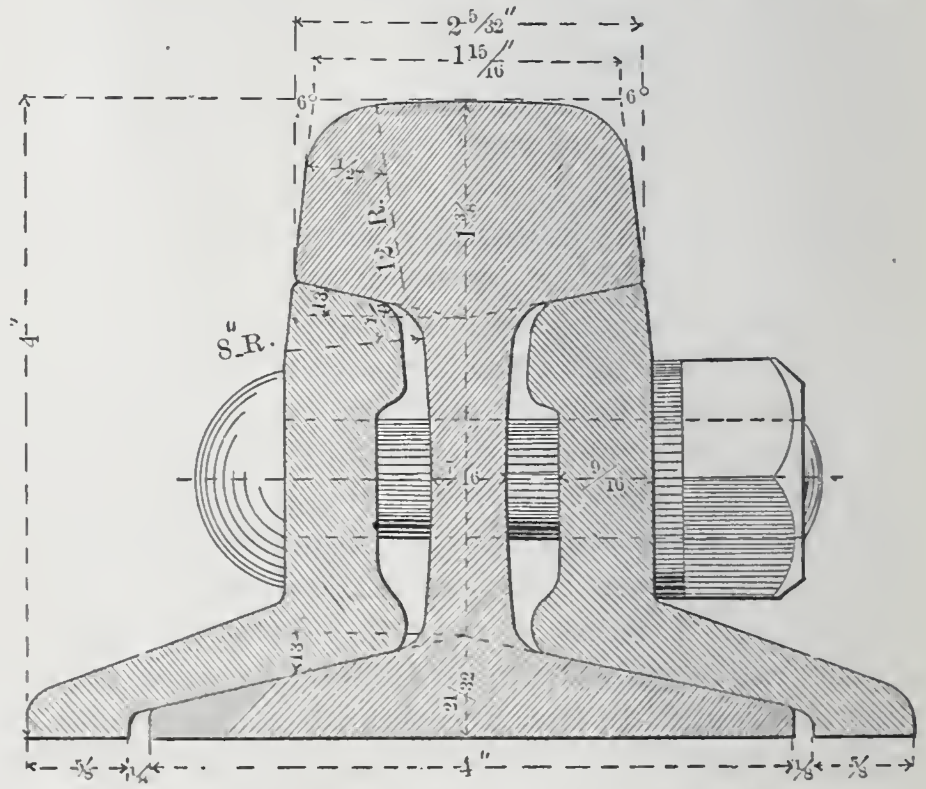
rail, I think every section should be designed so as to get the strongest fish bar possible, with the best contact surface. The base or flange of each section is made as wide as possible, consistent with good construction, and the flanges are, as a rule, wider than most sections in use, and I think sufficiently wide to prevent unnecessary cutting of the ties. These are the salient features of the new sections I have designed, and any one glancing at the sections will admit that, at least I have better distribution of metal than in the majority of rails now being made. I have thicker flanges, which guarantee greater tensility in the section, thicker webs with the neutral axis near where it should be, sufficient metal in head to do all that should be asked of the best rails, and when the rail is worn out, I think the record will be far better than that of those rails with thin flanges and webs and unduly proportioned head.

If a series of Standard sections were generally adopted by the railroads of this country, then each works could always keep on hand a duplicate set of rolls, and there would be no excuse for rough rails. As it now is, each year brings about a dozen or more new sections added to the already too numerous stock of rolls on hand and it is impossible to make catalogues fast enough.

The fish bar shown in each section is a slight modification of the design known as the "Sayre and Fritz" fish bar. From these 15 designs, 45 sections can be rolled. To illustrate, you can roll either 66, 67 or 68 lb. from the one pattern of 66 lb., or you can roll either 68, 69 or 70 lb. from the 68 pattern, thus easily giving a range of 48 sections from 15 standard patterns; in all cases the rolls can be raised to increase the weight of the section, but in no case would I advise decreasing the weight of the section from the standard given.

I think the mistake is made too frequently of spacing the bolt holes too far apart; for instance, the holes in the rails used by the P. R. R. Co., are 4 inches from end of rail to center of first hole, and 9 inches from end of rail to center of second hole. This leaves a space between the first holes of the joint of eight inches, which does not make a good joint. I think spacing the holes at $2\frac{1}{2}$ inches from end of rail to center of first hole, and 9 inches from end of rail to center of second hole, thus leaving but 5 inches between the two first holes will insure a far better joint.

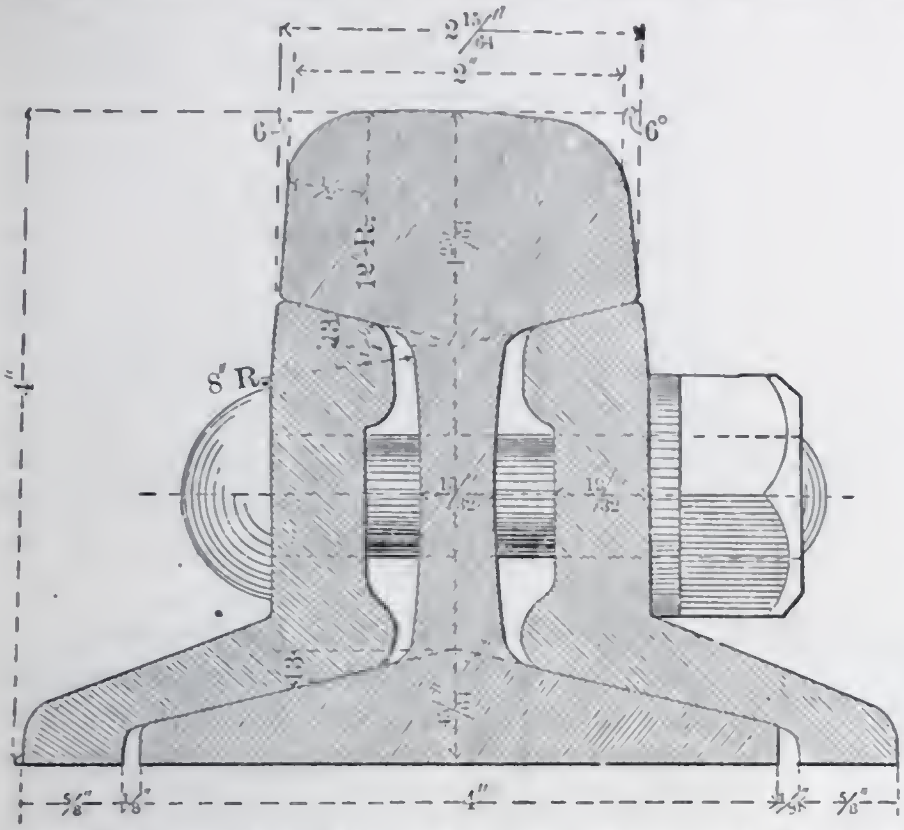
And a question for engineers to consider is: Whether the fish bar designed by Messrs. Bridges & Good, of the Western Division P. R. R., is not a step in the right direction. If we consider the usual suspended fish bar joint, we must recognize as a positive fact, as the joint is usually constructed, with fish bar supporting head of rail on two ties, at that point of the two ties



Rail, 52 Lbs. per Yd. Weight of Splice, 27 Lbs.

the rail is subjected to unusual conditions differing from the point of the rails or any other portion of the rail. Messrs. Bridges & Good

construct their joint by relieving the contact surface under the head of the rail, commencing at about 6 inches from each end of the rail, this forms a truss of the fish bar, transmitting the strains directly along the flange to the two ties, but obviating the very objectionable feature of each tie forming an anvil block for rails rest-

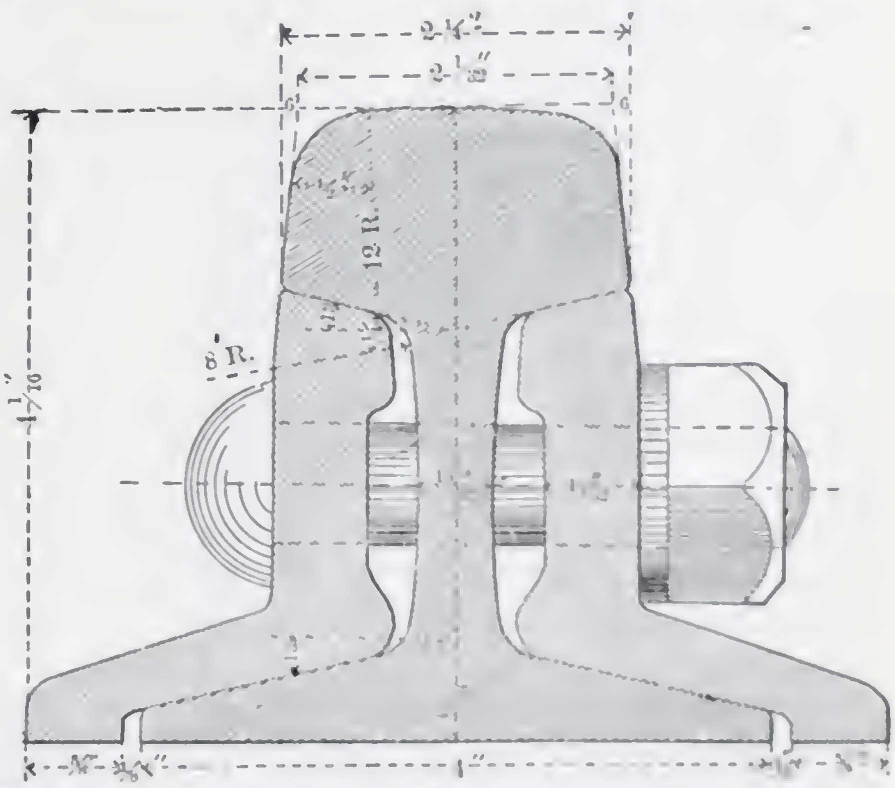


RAIL, 54 LBS. PER YD. WEIGHT OF SPLICE, 29 LBS.

ing directly over the tie. I think experience has taught every one that in ordinary fish bar joints, the upper side of head of rail and the upper contact surface of the fish bar for about six inches each way, shows in time wear with the old fish bar, it is not an easy matter to take up the lost motion, because the larger portion of the fish bar is not worn; by usingBridges & Good's joint the lost motion can be readily taken up. I am glad to say that this joint now on trial on the P. R. R., shows gratifying results.

I would call the attention of Engineers to the barbarous practice of drilling excessively large holes for bolts. I can see no necessity for drilling a 1 inch hole in the web of a rail. The C., B. & Q. R. R. have but a $\frac{7}{8}$-inch hole drilled, which gives them all the room required for expansion and contraction, and the model joint I exhibit clearly shows that a $\frac{7}{8}$-inch hole in web of rail, with a 13-16th hole in fish bar, is sufficient for all purposes, and to drill larger holes is, to say the least, erroneous and tending to weaken the joint.

From my own observation, and from knowledge I have gained from road masters, I know that a large percentage, in fact too large, of fish bars break in the track. I have seen two sections of track on two western roads where over fifty per cent. of the fish bars were broken. I have seen any number with angle portion of the bar broken and the angle lying along side of the track. This has grown to be a shameful evil, and it is evident that one reason is that the fish bars are not properly designed to meet the requirements of actual service, and, it is also evident that the material used in making the bars is totally unfit. Why railroad men do not use steel for fish bars in place of iron, is to me, an enigma, as it is a well known fact that steel is nearly equal in strength in all directions, while iron has its greatest strength in the direction of its fibres. As an illustration; the mechanical engineer who would build a boiler of iron and have the grain or fibre of the iron running longitudinally would be considered an ass, while with steel this would make but little difference. The fact that fish bars frequently break at the junction of the angle with the vertical part of the bar, proves that in that direction there is but little strength, while if the bar was made of steel, I do not believe that a single case of fracture would occur at that point. If an iron fish bar is made of good iron, then its tensility is too low. If made of hard, granular iron, showing fairly good tensility, then its brittleness renders it worthless. Why engineers in this country should use iron fish bars instead of steel, when engineers abroad have abandoned the use of iron fish bars, where with a little experience, a steel fish bar can be made as cheaply as a good iron one. I should think the same argument, why a rail should be made of steel in preference to iron, would, or should, hold

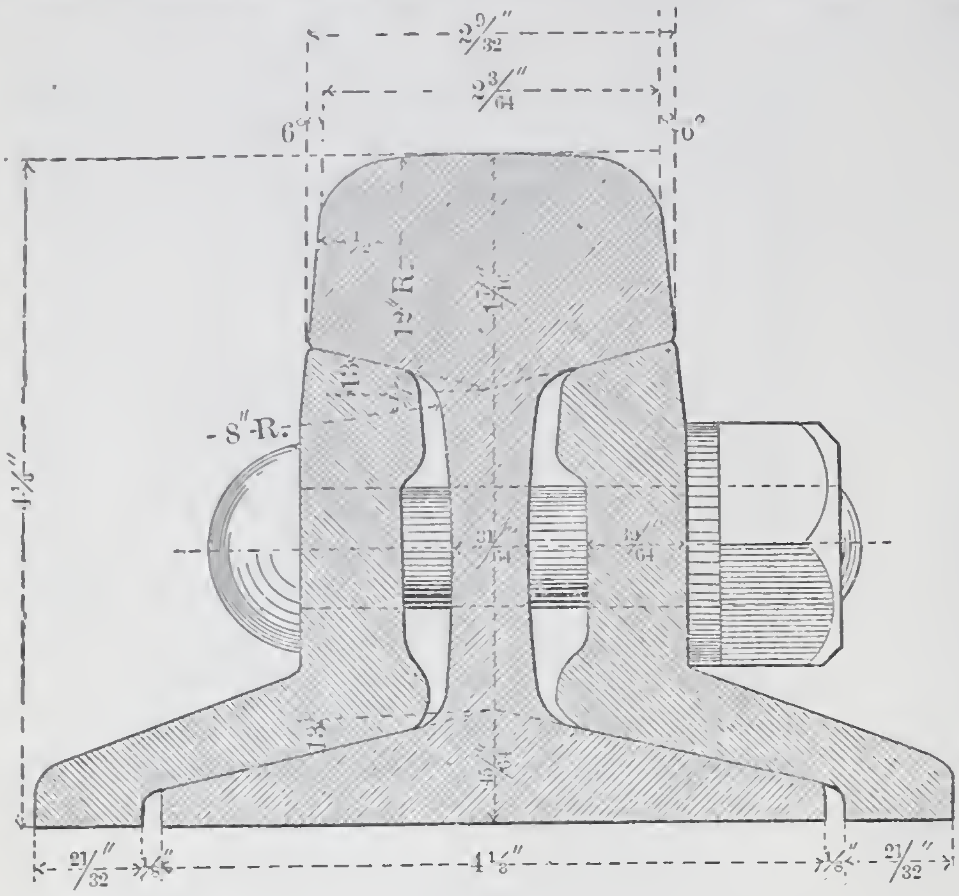


RAIL, 56 LBS. PER YD. WEIGHT OF SPLICE, 30 LBS.

good with fish bars. At the very least, I should say that the continued use of comparatively worthless iron fish bars would seem to indicate a spirit of non-progressiveness on the part of those in charge of the maintenance of way on our railroads.

DISCUSSION.

Mr Jones:—I would like to have some of our railroad friends take this subject up. I would like to illustrate now the drilling of bolt holes.



Rail, 58 Lbs. per Yd. Weight of Splice, 31 Lbs.

They are made too large as a rule. (Here he took several templates and also referred to his drawings to illustrate.)

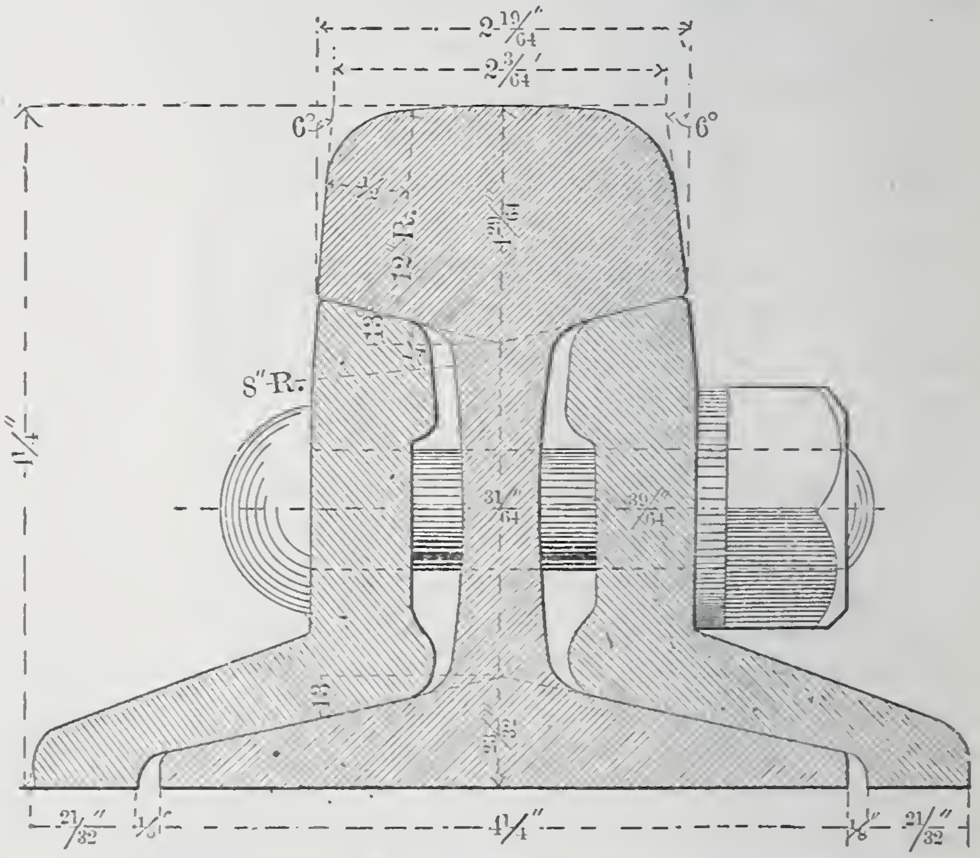
This represents a joint made to open in cold weather, showing a 7/8" hole drilled in web of rail, with joint wide open. This other, however, shows the rail closed in hot weather. You can see the space left. This is an illustration of the Bridges & Good joint.

I take it the surface of the fish bar is 4 inches, from here (the top) to the center, and from there the fish bar is relieved. So that, as every railroad man knows, in actual service the head of the rail wears upwardly while the fish bar wears downwardly. If you attempt to tighten it your fish bar here has no wear and is a solid mass and is held right tight over the tie, and the load passing makes it form an anvil block. If your joint is loose you can readily take up the lost motion.

The Pennsylvania Railroad joint does not support the head of the rail at the point necessary. Any engineer looking at a train passing over here will notice that the transmission of the strain is right through the head to the tie, the fish bar having no function to perform. In this other case we have made a brace to support the load.

This joint here is an illustration of the idea I brought forward the other evening. This one is practically an illustration of the drilling of a 13/16" hole in the splice bar and a 7/8" hole in the rail. Some engineers drill a 1 1/16" hole, which destroys the efficiency of the fish bar. With such holes I have seen joints with 3/4 of an inch space. If the holes were drilled properly it would not give too large a space. As a manufacturer of steel rails I say the holes ought to be drilled, to obviate that bungling hole we discussed the other evening, carrying out the idea of making the bolt in that form (showing sample) and slotting two half round holes to keep it from turning. There is no difficulty in putting the bolt in. In other words, it is an attempt to add a little refinement to the railroad track.

Now, another thing. I have no hesitation in making this declaration before this meeting, as a rail maker. We are often asked, in talking with railroad men "Why don't you make a rail like John Brown's steel rail?" I make this assertion, that no manufacturer of American rails to-day will make as brittle and as dangerous a rail as those furnished by John Brown. For American railways 60 per cent. of those rails are entirely unsafe for the track, and I have no hesitation in saying it. If I knew of a road in this country

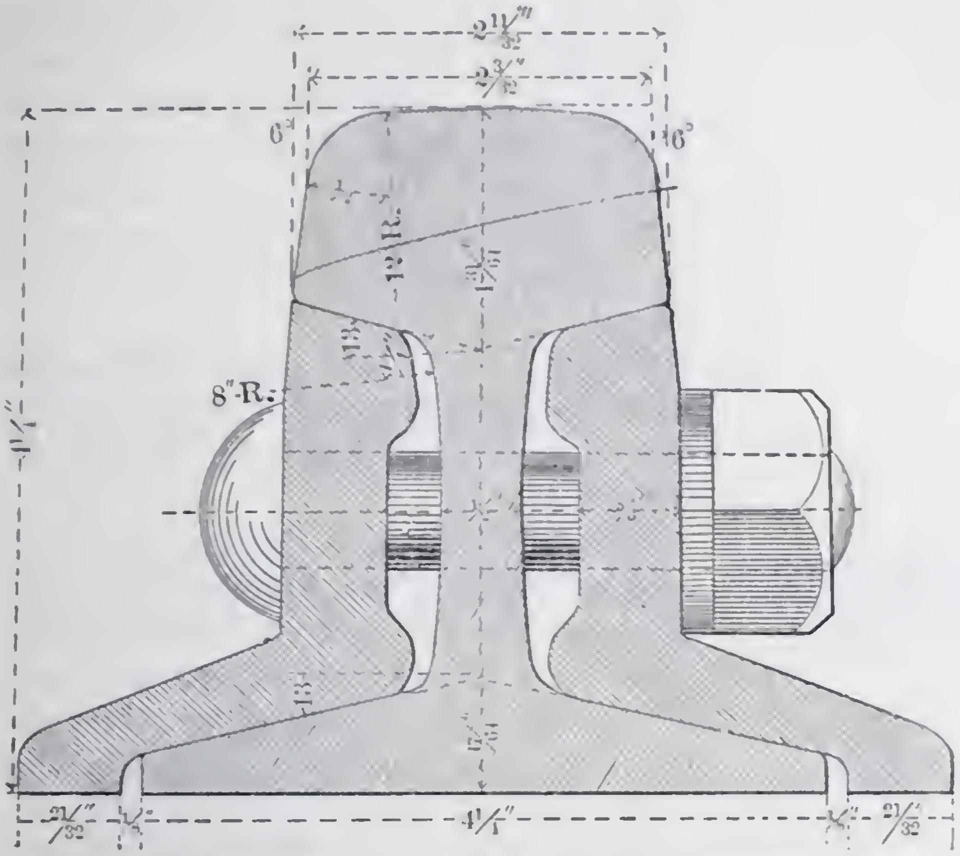


Rail, 60 Lbs. per Yd. Weight of Splice, 32 Lbs.

laid with such rails I would not want to go over that road.

Any rail, I do not care who makes it, by the Bessemer process, that contains one per cent. of

carbon, as high as .3 to .4 of silicon and as high as .18 of phosphorus, is unfit for railroad purposes. The fact is beginning to leak out that a large number did break in the track. That is an ab-



Rail 62 Lbs. per Yd. Weight of Splice, 33 Lbs.

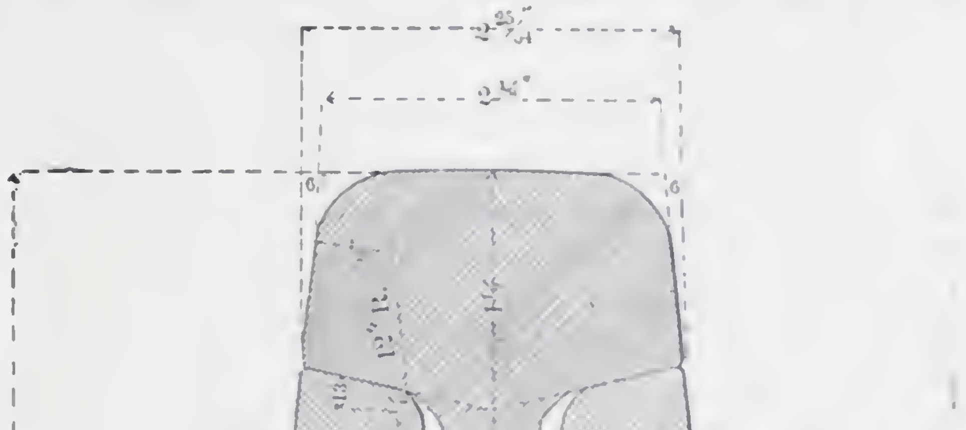
solute fact. The chemical analysis of these so-much talked-of English steel rails will not compare at all with the average American rail. And I have no hesitation in saying that if the English rails of which we hear so much boasting, had been made of same design as modern sections now in use, and had been subject to some enermous loads and traffic that American rails are subjected to the list of frightful accidents would have been greatly increased. The fact that those English rails were made with very thick web, and flanges, as a rule not exceeding $3\frac{3}{4}''$ in width and averaging $\frac{7}{16}''$ at point of flange, has saved the English rails from causing many an accident. No American maker of rails to-day would dare to risk his reputation in making rails as brittle as many of those furnished by English makers.

Mr. Becker:—I think Mr. Jones is right with regard to the frequent excessive amount of material in the heads of rails; whenever you have obtained a section which fulfills the requirements as to stiffness and strength, all that you want in addition to that is to have a supply in the head of a sufficient amount of metal, over and above that required for strength and stiffness to compensate for the constant wear.

But I think there has been entirely too much expectation as to duration of a rail, on account of its wear. My experience has been that no matter how large the rail head is there is only a certain amount of it that is actually fit for wear, and when that certain amount is worn out the rail of a larger section is just about as unfit for additional wear as that of a smaller section. Therefore I do not think that it is best to exceed a certain limit in the top of the rail sections.

Of course, in addition to this constant area that you ought to give, in order to satisfy the requirement of strength and stiffness, there is this other factor to be provided against, and that is the constantly increasing loads that are pushed on to the railroads; but even taking that into account I think there is a tendency manifested in the manufacture of rail sections to increase the head of the rails unnecessarily, and I think that even Mr. Sayre, who has certainly shown a great deal of judgment in his sections, has gone a little too far in the designing of his rail sections, merely for the accommodation of his, in my estimation, excessively large angle bar sections.

I think that as far as the wear of rails is concerned that if it is a proper thing in wheels to condemn them after they have run a certain number of miles, it would be just as proper to condemn a rail after carrying a certain amount of tonnage.

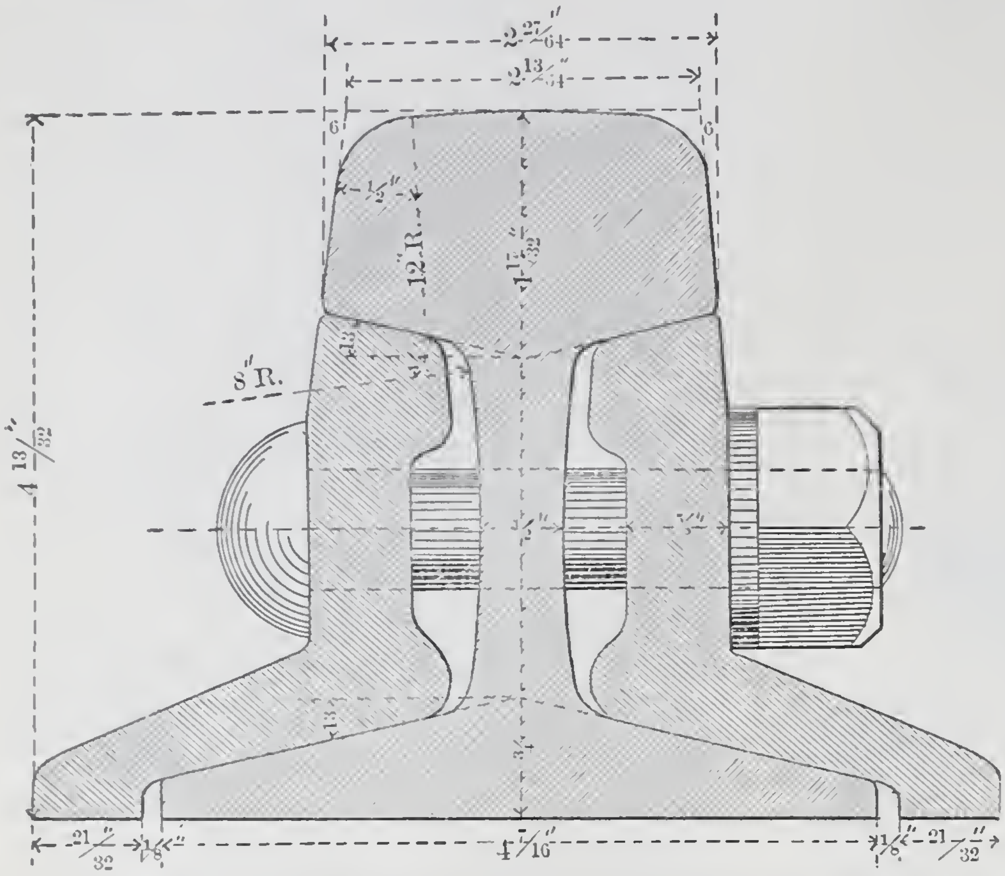


Rail, 64 Lbs. per Yd. Weight of Splice, 34 Lbs.

As to the propriety of introducing steel for angle bars I think if Captain Jones will stick to his word and manufacture them as cheap as iron he will have no trouble in getting all the orders

he wants. We cannot get angle bars of steel as cheap as of iron. I fully concur with him that that is the kind of metal to put into a track.

Mr. Dempster:—What is the wearing effect of wheels on rails?



Rail, 66 Lbs. per Yd. Weight of Splice, 35 Lbs.

Mr. Becker:—When it comes to wheel manufacture I must say that I am so sceptical as to taking the assurance of manufacturers that I do not know whom to believe any more. There has been a large amount of literature published lately in regard to the necessity of fitting the rail sections to the wheel flanges.

Mr. Forney has sent circulars all over creation and has gotten some replies to them but he has chiefly obtained his information from the master car builders and people connected with the rolling stock of railroads. He has asked very little information from the men who have charge of the track.

If I recollect aright, the drift of his argument was this, that railroads were made to carry traffic and therefore the rolling stock was the dominant feature and everything else connected with a railroad had to follow suit.

The next point in his argument was that the flange angle between the tread of the wheel and the flange proper, had to be made with a radius of not less than ⅝ of an inch, for the reason that the chilling could not be properly done with a sharper angle.

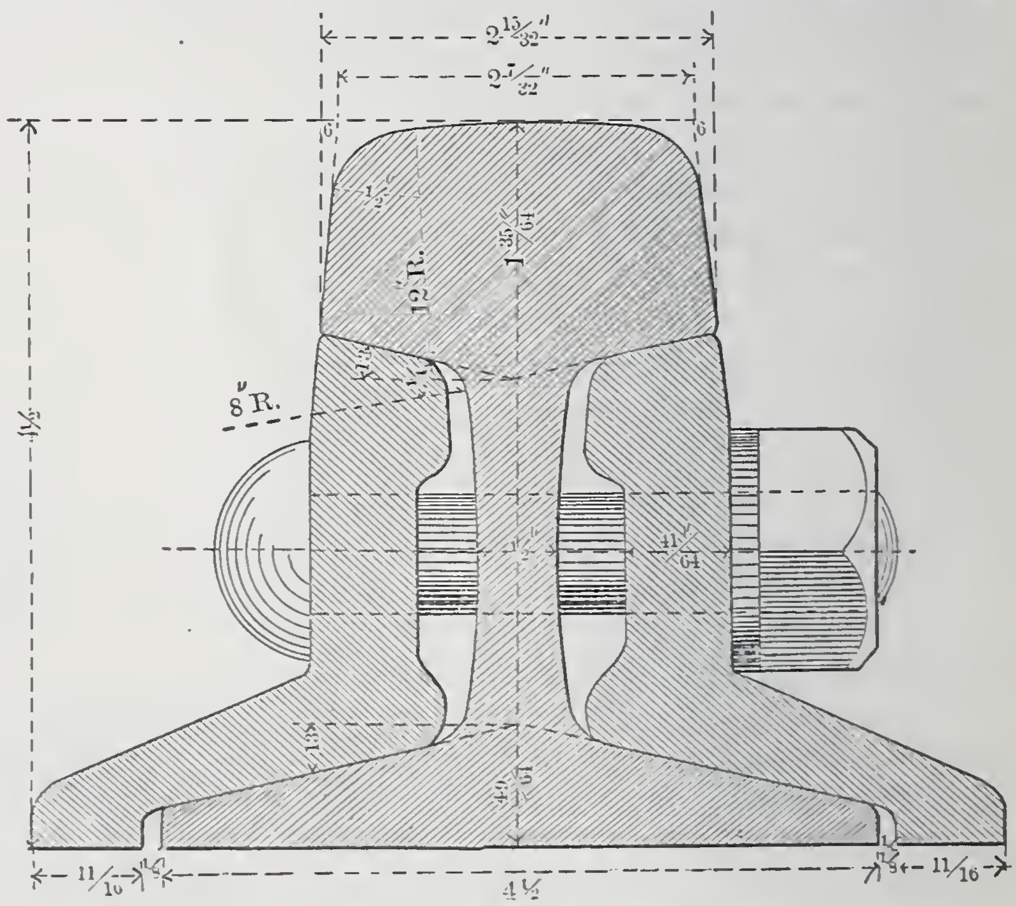
Now, I do not know whether it can be done or not and until I do know something about that subject I do not want to express an opinion. On the other hand I do not believe that the conformity of the flange to the rail is a matter of any very great importance. It looks to me like going into a refinement of the subject that won't wear longer than a few days.

As to the wear I think there is as much wear of the rails on one side of the track as on the other. I have seen rail sections on sharp curves diminished fully 33 per cent. in the head; that was evidently done by the wheel and I have never seen anything like an equal reduction in either the flange or tread of a wheel.

Mr. Jones:—I just want to illustrate how this useless multiplicity of sections occurs. (He here displayed a number of templates.)

This template is a standard of 60 lb. rail, adopted by the Pennsylvania Railroad back in 1875. From that one standard there have grown some 18 sections, all intended to represent that one section, but one engineer has made that angle 12½, and one 14, but all with the same intention, to represent the original section.

Another illustration: Take this from a prominent railroad engineer. We have rolled probably 200,000 tons of the rail shown by this template, representing one of the standard sections. The engineer of that road came up with an entirely different section. I told him we



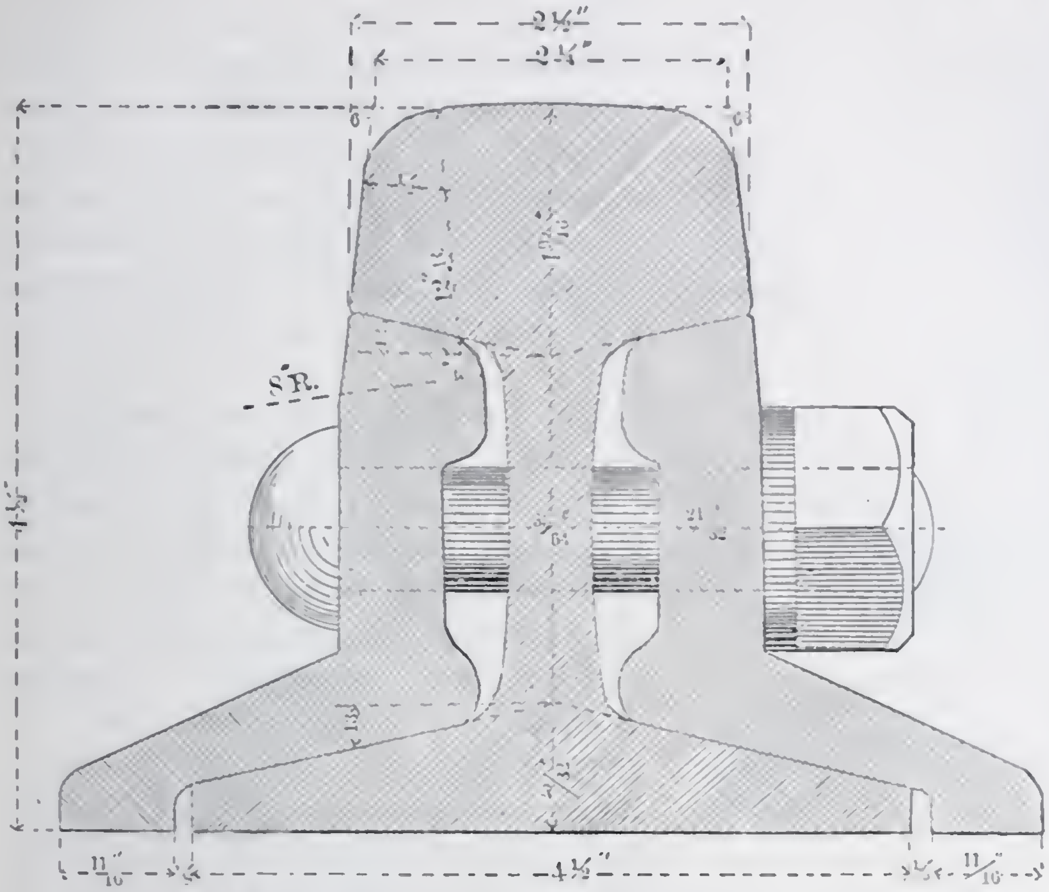
Rail, 68 Lbs. per Yd. Weight of Splice, 37 Lbs.

had the original section, that is what he was using now. The section offered by him was entirely different.

I wrote the engineer asking if he would not modify the section and he replied that it could

not be disturbed, which is considered an impertinent reply.

This template was sent to us to take it up. It is not true. There is a different radial line here. It is not square or anything else.



RAIL, 70 LBS. PER YARD. WEIGHT OF SPLICE, 33 LBS.

Here is a template sent us that would not fit into the gauge anywhere. You can see easily all through it and the other side is still worse. That is a sample of the difficulties we have to meet. We have an order for a rail like that (showing template) and required to guarantee it 5 years. I would not guarantee it 5 months, or rather I will say I will guarantee that section as long as it lasts and no longer. The section is not fit to be rolled. The web is so thin that we cannot keep rail from twisting.

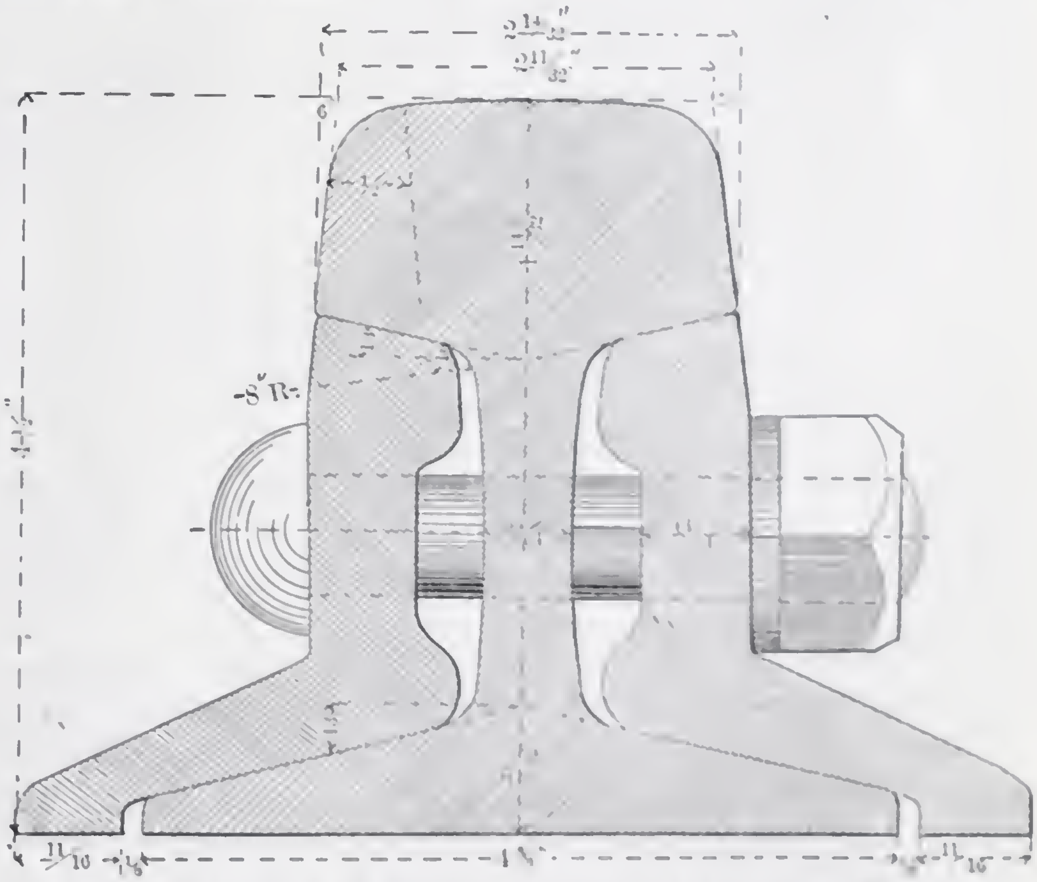
Now, I cannot see any reason why a series of standard sections cannot be adopted by the different railroad companies. Here is a point to illustrate. Suppose every engineer in bridging and other structures would make his own designs as to the shapes to be used, say 11 or 13 inch beams or channels, what would be the result? There are standard sizes given for such work and engineers simply get a catalogue and make their calculations and go and construct their bridge in relation to the standard sizes. I want to get engineers to understand that question, that there is a range of rails, and if they dont like the forms for heaven's sake call a convention and get something definite and do away with the excessive cost arising from our being compelled to keep so many different sets of rolls. There is no excuse at all for that and why it cannot be done I don't know.

Mr. Bridges, the engineer who designed this joint is present. I would like to hear from him.

MR. BRIDGES:—I think it best not to say anything to-night, but at some future time I would like to read a paper on it, but I cannot say more than has been said. I think Captain Jones has covered the ground very well to-night.

With the permission of the Society at some future meeting I would like to read a paper on the subject, provided in the meantime I become a member of it.

MR. KENT:—Captain Jones has to-night made a growl about those people that buy rails not specifying his particular sections. I heard him make that same growl at Philadelphia several years ago. I think at that time I advised him to get up a section and call it the Edgar Thomson section, or preferably, the Jones section, and sell it at one price, and if any one wanted a different section, sell it at a higher price. It would be a great advantage both to the producer and the consumer. I have not seen the Captain do that.

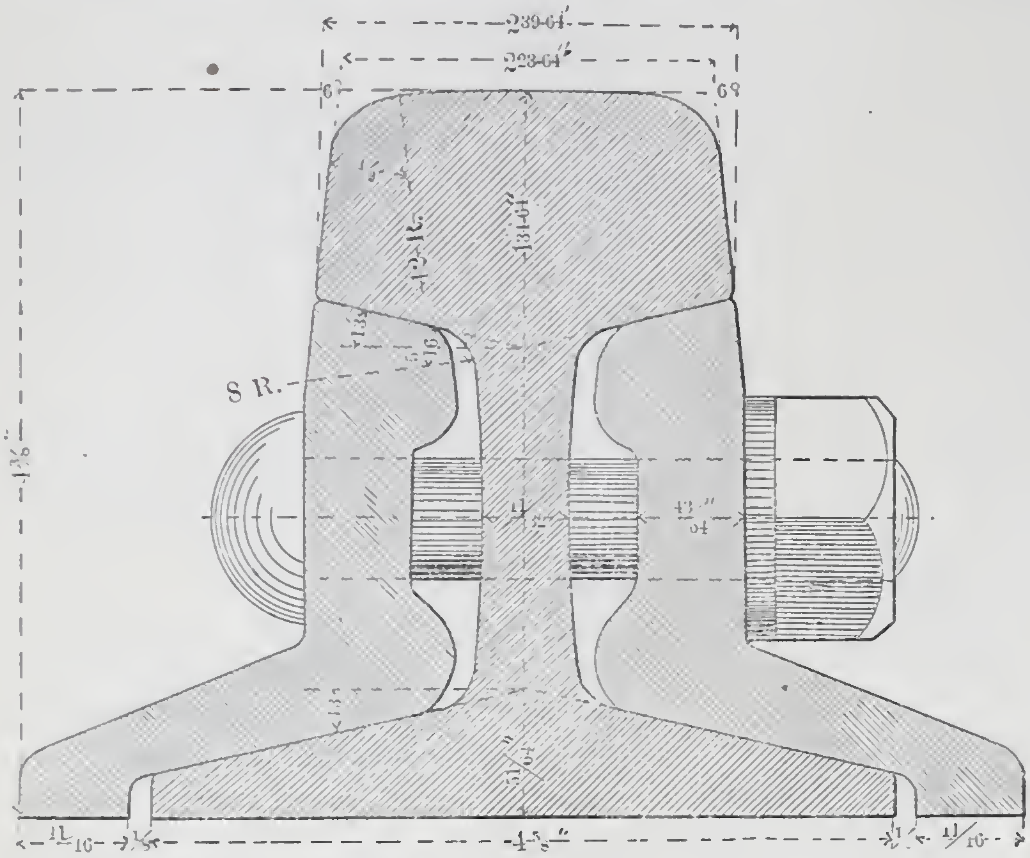


RAIL, 72 LBS. PER YARD. WEIGHT OF SPLICE, 39 LBS.

MR. JONES:—I suppose the additions will average about fifteen new sections every year and we are compelled to send rolls back that are not a quarter worn. It is a peculiarity about rail-

road engineers (I believe the engineers present are exceptions) that each man insists that he shall display his ability in designing new sections.

Mr. Becker:—On the subject of those holes,



Rail, 74 lbs. per Yard. Weight of Splice, 40 Lbs.

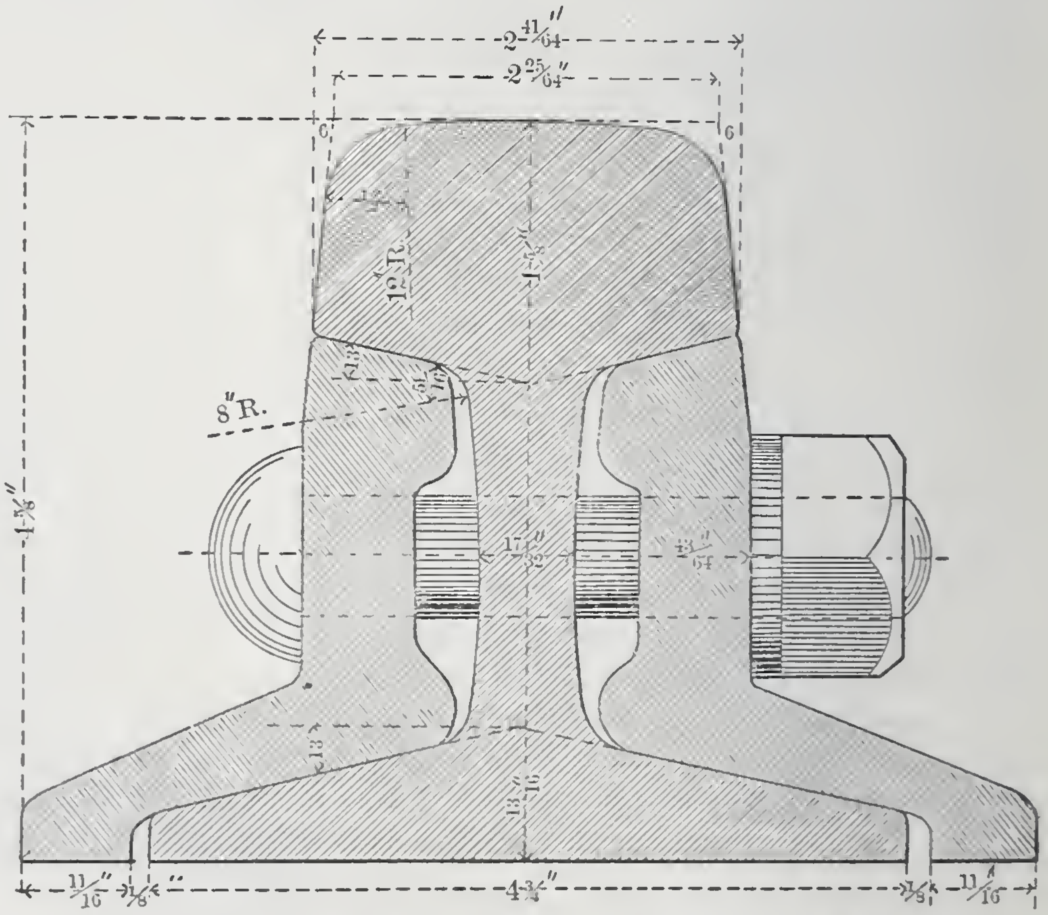
there is a tendency to uniformity in railroading, perhaps more than in any other branch of business. It used to be that splice bars were punched on the one side with a circular hole while the other one was an oblong hole. We had trouble occasionally, however, from the Irishman getting the thing reversed and it led to delays. You had to carry your splice bars in pairs. But even then when you would send a man back to get splices, perhaps a mile from where you were working he was liable to come up with two splices for the same side, or with one of one kind the other of another; and it became necessary finally to punch both sides oblong so that the Irishman would make no mistake. Sometimes it was necessary to teach him the difference between an oblong and a circular hole.

I went over a railroad last week, the Scioto Valley, where they had reversed the thing; they had a square straight bar on one side of the rail and an angle bar on the other, but unfortunately they punched the things vice versa and they had to put the angle bar on the inside of the rail. I could not understand it and inquired the reason and they told me that the fellow that ordered the bars forgot to designate the holes he wanted. I suppose this was blamed on the railroad. If the engineers presumed on the intelligence of the mill they certainly got fooled that time.

Mr. Kent:—I do not think you have as good railroads in this country as in England. You have not a chair under the rail. What particular reason is there for American roads using angle fish plates, while the English use a chair. Is there any particular reason why we stick to this form of the chair? Is there anything in the claim of weight, or what is it? English roads are better built and stand higher as a rule.

Mr. Jones:—That is easily answered. On the standard roads in England the standard section of rail is 84 lbs. per yard. If that is the correct section there we should use about 120 lb. rail here. Their locomotives are comparative pigmies alongside our moguls and our passenger engines. You take an English passenger train and their whole train will hardly weigh more than one of our Pullman cars. This is true not only of the English but of the German and French roads. Their load per wheel is very slight compared with the load per wheel of an American road.

A great deal of the trouble existing here to-day



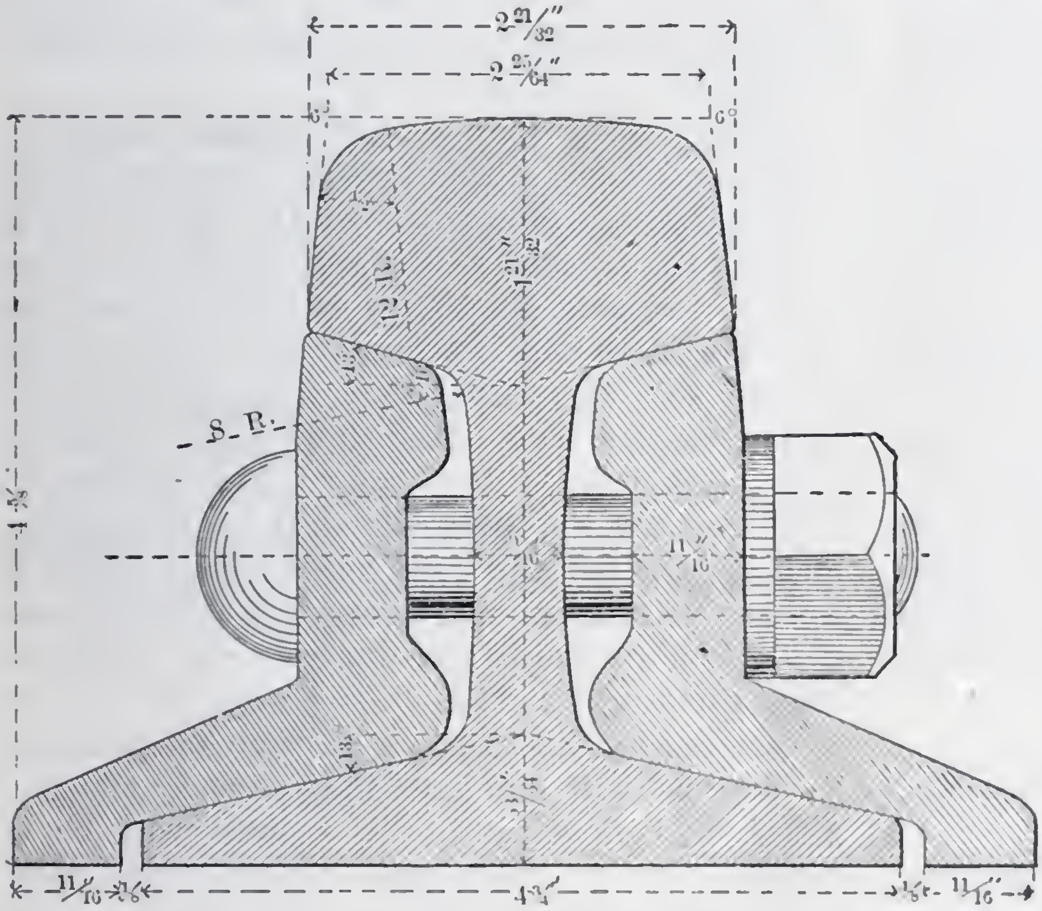
Rail, 76 lbs. per Yard. Weight of Splice, 40 Lbs.

is the very point Mr. Becker mentions, that every railroad engineer does not consider a very important thing in the designing of rail sections, viz: their strength and safety. In the number of

rail sections we roll they will order 50-lb. rail for traffic that a 60-lb. rail at the least should be used for. I take into consideration this question, that if this rail breaks when it is used beyond its capacity and lives are lost thereby, the responsibility is thrown on our shoulders. It is a very serious thing to put on the shoulders of the rail maker. Our rail sections in this country, as a rule, are unfit for actual service.

The Sayre section has an excessive mass of metal in the head and has comparatively a thin web and thin flange. He has not satisfied the condition of safety at all, but wants great service out of the rail. The average life of a rail should be about ten years.

Mr. Becker:—The English have adopted larger rail sections on account of the fact that the cost of cross ties is getting to be an important item and they do not space them as close as we do here. I do not agree with Mr. Jones, that if an 84-lb. rail is the proper thing for them, we should



Rail, 78 lbs. per Yard. Weight of Splice, 41 Lbs.

have here 120 to 150-lb. rails. On the contrary, because we lay ties so much closer, a lighter section will answer as well here as heavy section over there. We have very little trouble with rails actually breaking and we have very little trouble about their bending. They go by lateral flexure rather than by outright weight. That ought to be sufficient to show that ordinary sections, 67 to 72 lbs are good for present traffic purposes at least, as long as we are spacing ties as we do now, 16 ties to the 30 foot rail.

Mr. Jones:—I do not know, Mr. Becker, in the railroads of England I found the ties pretty close. Then they have pretty level track over there. They do not run up a hill then down into a deep valley or around a hill when they come to it, as we do, but straight through it. If you take the Pennsylvania road, for instance, you have not many miles of straight track between Pittsburgh and Philadelphia. On curves, to carry the heavy freight and passenger loads in this country, at least on the trunk line roads, I think the sections should go up to 76 lbs. Of course I would be interested in heavier sections because we would get a bigger tonnage, but if a number of years ago a 65-lb. rail was the proper standard for the Pennsylvania Railroad, to carry 15-ton cars, then 76 lb. is not too large to-day for 20-ton loads.

Mr. Strobel:—They space cross-ties one metre on the continent.

Mr. Becker:—There is a general characteristic on the part of John Bull. He wants everything multiplied by two, to make it safe. They would not travel in any buggy such as we use here. They want big clumsy vehicles for everything. The same thing is true on the continent.

Mr. Dempster:—Why would it not be a good idea to decide on different sections of rails, catalogue them by numbers and send them to the engineers and let them decide. The manufacturers would not then have to have so much machinery for the making of rails and it would not be necessary to carry so many rolls.

Mr. Jones:—It is impossible for any manufacturer to carry even a ton of rails in stock because you do not know when the engineer will change. Different men will have different sections. It is impossible for the rail manufacturers to carry one single ton of rails.

If standard sections are adopted the manufacturer could afford when orders are scarce to make stock. For instance, then, when it is hard to work, in excessively hot weather, men need not work as the rails would be on hand. In winter you could manufacture when ordinarily you have no orders. For standard sections they could roll in winter and carry the stock in summer. You could pick up any number of orders for 500 to 1,000 tons in that way.

Mr. Becker:—Let me suggest another thing. There is a tendency among all classes of people to save themselves labor. I know that most railroad engineers have lots of work on hand and they do not want to sit down and take a pencil and square and design sections just for the fun of it. I never saw one yet, while on the other hand from the Cambria Iron Works and Edgar Thom-

son Works they get a beautifully bound volume sent gratis each year, each volume containing about 200 to 300 rail sections. The engineer finds it a great deal easier when he wants anything in that line to send and say send me No. so-and-so.

I think much of the difficulty complained of would be avoided if you would cut down your sample book to a few standard sections.

Mr. Zimmermann:—Has any concerted action ever been taken in regard to standard rail sections?

Mr. Jones:—Never.

Mr. Brown:—Such has been done regarding standards for cars and couplings.

Mr. Dempster:—Couplings are patented. Sections of rails are not, and each man has a patent and wants to realize cash out of it.

Mr. Brown:—There is one thing I would say (referring to section of a rail shown by Mr. Jones) that the man that designed that rail never was on a railroad in his life. He has probably gone into partnership with some spike factory. The way that is there you would have your tie entirely cut out for the spike cuts through. To have it cut off square is to me the best idea. The curve acts as a chisel and cuts the spikes through. That is entirely too sharp.

Mr. Jones:—Roadmasters say they use 60 per cent. more spikes now than with the square flange rail.

Mr. Brown:—Another thing is that whenever a rail is broken it is so transversely across the first bolt. Now why is that? It is because, as Mr. Jones has said, the spacing between the two bolts allows it to spring. Here is the weakest point, the first bolt. I never saw a rail that did not break at the first bolt hole.

Mr. Dempster:—Run it closer!

Mr. Brown:—That was suggested by Mr. Jones.

Mr. Roberts:—Is the inside fish plate more likely to break than the outside?

Mr. Brown:—I do not know. I never noticed. They generally break through the first bolt hole.

Mr. Becker:—They break at the joint, between the two holes.

Mr. Kent:—There has been something said about not having standard sizes of rails. Now the master car-builders have adopted a great many standards and some of them are entirely successful, such as the standard axle, the standard journal. They adopted a standard for screw threads.

Now, it takes some time to work these things. There is no society for the maintenance of way, as the car builders and mechanics have, and I think therefore this society is the proper one to deal with this subject. I think it would not be unwise to appoint a committee to report, the committee to consist of engineers of maintenance of way and manufacturers. I mention that Philadelphia meeting again. There was a very considerable discussion on the subject but nothing definite came of it. Nothing was done and here we are, four years later, in the same condition as then.

Mr. Jones:—(Referring to the tracings of sections) in designing these sections I conversed with many of the best roadmasters on the subject. I got their ideas as to what should be done. And while, therefore, many of my own ideas are represented, there also are ideas there of some of the best roadmasters in the country.

As a rule they are opposed to this unusual distribution of metal in the head, and excessively thin flanges and web. I have derived a great many ideas from the men who are maintaining the track. They uniformly condemn the excessive space between the heads. The further that you may have that hole the weaker you will have the track.

Another thing in connection with maintaining a good track, is that many roads require rails to be slightly cambered. This is wrong and if any camber is to be in the rail it should be exactly the reverse. In other words, if rails were laid in track with ties perfectly level, then rails should rest on center ties, with ends slightly above the ties. For illustration I make the statement that from Pittsburgh to Philadelphia on the Pennsylvania Railroad the track is a succession of cambers and low joints, and I doubt if but few railroad men are aware of the fact that the action of trains passing over rails will produce the camber. Yet it is a well known law in mechanics that rolling friction on a metallic surface will elongate that surface and the metal will flow, and the results will be a camber. So it is with rails. The action of passing trains will elongate the metal in head of rail and the inevitable consequence is a camber, and this fact will partly explain the difficulty of overcoming low joints.

Mr. Roberts:—In 1864 our old chief engineer used on a line of track we had wooden joints. There had been some trouble, but after he made the substitution, although a part of the rails were on a bridge and we had heavy trains there I never saw a broken fish plate on that division. I frequently walked over it.

His idea was that the flanged surface helped to give more weight on the inside of the rail than outside. For that reason I asked the question if the records show which fish plate is apt to break.

Mr. Becker:—If there is any difference in the

breaking of the angle bars the outside bar on curves is more apt to break. They get a little additional stress at the joint and are more apt to break on that account. On straight track one is as apt to suffer as the other.

Mr. Brown:—The tendency of the track men is to raise the ties at the joints. They will insist on having them higher at the joints than the rest of the rails. They tamp the rails from the outside to the inside. It should be tamped from the inside and not from the outside, and in consequence it gives a bearing on the outer end of the ties, which sends the track down again.

Mr. Dempster:—In regard to carrying out the suggestion of Mr. Kent, would it not be well enough to ask this committee on splice bars to extend their investigations further, to rails, and formulate a series of questions to be sent out by the secretary to get information from chief engineers of the country to give their ideas of the cross sections of rails?

Tests should be made of the strength of the rail, for the purpose of comparison with the strength of the joint; but we do not want to know the strength of the angle bars alone; what we do want to know is the strength of the complete joint just as it exists in the track. It will not answer therefore to sandwich two short chunks of rail between the angle bars and test that combination, as the short rail pieces would simply go down under the test and offer no resistance, except what little strength there may be in the bolts. The rails to be tested with the angle bars as a combined joint, should be sufficiently long to obtain the advantage of their continuity, or if that is impracticable they should be clamped or fastened in such manner as to obtain results similar to those obtained from testing a joint with long rails inserted between the angle bars. The result of a series of such tests, applied to a variety of rail sections and to a variety of splice joints would certainly be very interesting, and if made known to the parties in charge of railroad track, would undoubtedly result in promoting uniformity in the sections of rails and splices and in a reduction from multiplicity to uniformity it is quite reasonable to predict that the fittest will survive.

Mr. Strobel:—I think that the question of standard rail sections, which is the one considered to-night, is entirely distinct from the strength of rails. Of course, in testing rail joints it is necessary to use rails in connection with them, but even if we know what strength the rail has we do not know whether it is a good section for adoption on railroads. And a great many other questions have to be considered, wear, etc.

I think I can speak for the committee on rail joints that they have already a field for operations that is exhaustless and I think it would not be advisable to include also standard rail sections. Besides that committee, I think, has only one of its members who is connected with maintenance of way.

A committee that should form standard sections for adoption I think had better operate distinctly from the committee on rail joints.

EARTHWORK—THE PROFILE OF QUANTITIES.

[A paper read before the Engineers' Society of Western Pennsylvania, September 15, 1885, by S. B. FISHER.]

It was my fortune, years ago, to have access to a manuscript translation of an article entitled "*The Profile of Quantities.*" It purported to be founded on a chapter of Cuhlman's Graphical Statics. This article was prepared by a German engineer, apparently as an exercise in English composition, so that it presented an idea often, in a manner which to one born to the use of English, seemed not as if it walked in head foremost on all fours, but rather as if pulled in, in spite of itself, by the tail. But in this, as in many similar cases, after having mastered these peculiarities, and adapted it to American practice, we find it very handy and very complete.

To illustrate the subject, let us take an example from actual practice, section No. 7, of a road built two or three years ago.

Before constructing the profile we must make a tabular statement. This tabular statement has six columns. In the first column are the station numbers. The distance between these stations is usually 100 feet, but may be any other unit of measurement which is used in the construction of the work. The section of work here shown extends from stations 638 to 710. In columns 2 and 3 we have the total excavation and embankment within the station. These quantities are considered as distributed uniformly over the station. They are obtained from the estimate sheets. Figures of excavation are written with blue ink, those of embankment with red ink. In column 4 we have the quantities deposited within the station, which is obtained from 2 and 3 in an obvious manner. Unless there is a special price for this work, deposited by casting and wheeling, this column may be omitted. If casting and wheeling have different prices, two columns may be used. In columns 5 and 6 we have the final ordinates, which are obtained by cumulating the increments in columns 2 and 3, by adding like and subtracting unlike signs or colors. The final ordinate should be equal to the difference between the sums of columns 2 and 3, and should be in favor of the excess of excavation or embankment as the case may be.

The *Profile* is made by plotting the ordinates in the last two columns, on ordinary profile paper. The blue is taken as plus and the red as minus. The vertical scale is selected as any other scale, depending on the accuracy required for the work, size of the paper, etc. The points obtained by plotting the ordinates are now connected by lines. A descending line is shown on the profile in red, and an ascending line in blue. The red thus always represents embankment and the blue excavation; because in going from one station to another, if the embankment is in excess, the increment will be negative and the line of profile will incline downward, the reverse being true for the blue.

And now comes the most important line of the profile—the *Balancing Line*. The principal business of this line is to determine the sub-sections. Let us look at the profile to see how it does this and other things.

One point through which the line must pass, is the beginning of the profile. The next controlling point is at station 653 + 40, where there is a run which was not crossed with material. Connecting these points by the line $A\ B\ C$, we have the first part of the balancing line. The inclination of the balancing line gives a graphical representation of the shrinkages, or as it is in this case, the expansion. At station 652 — 3 we have an embankment which is made from the excavation 651 — 2. The main embankment, 638 — 644, is made from the excavation 644 — 651. Prolonging the inclined line from $A\ B$ to its intersection with the horizontal through F, we have the pole P. Lines lying in the angle between these lines and passing through P, will cut equivalent segments from the red and blue curves. The most important one of these lines is the line indicating the *Limit of Haul*. This limit of haul on this section was 800 feet. For every cubic yard hauled 100 feet over this distance, the contractor received an additional compensation. $P\ G\ H$, having $G\ H$ in horizontal projection 800 feet long, fulfills this condition. Passing polar lines through each point of

PROFILE OF QUANTITIES—TABULAR STATEMENT.

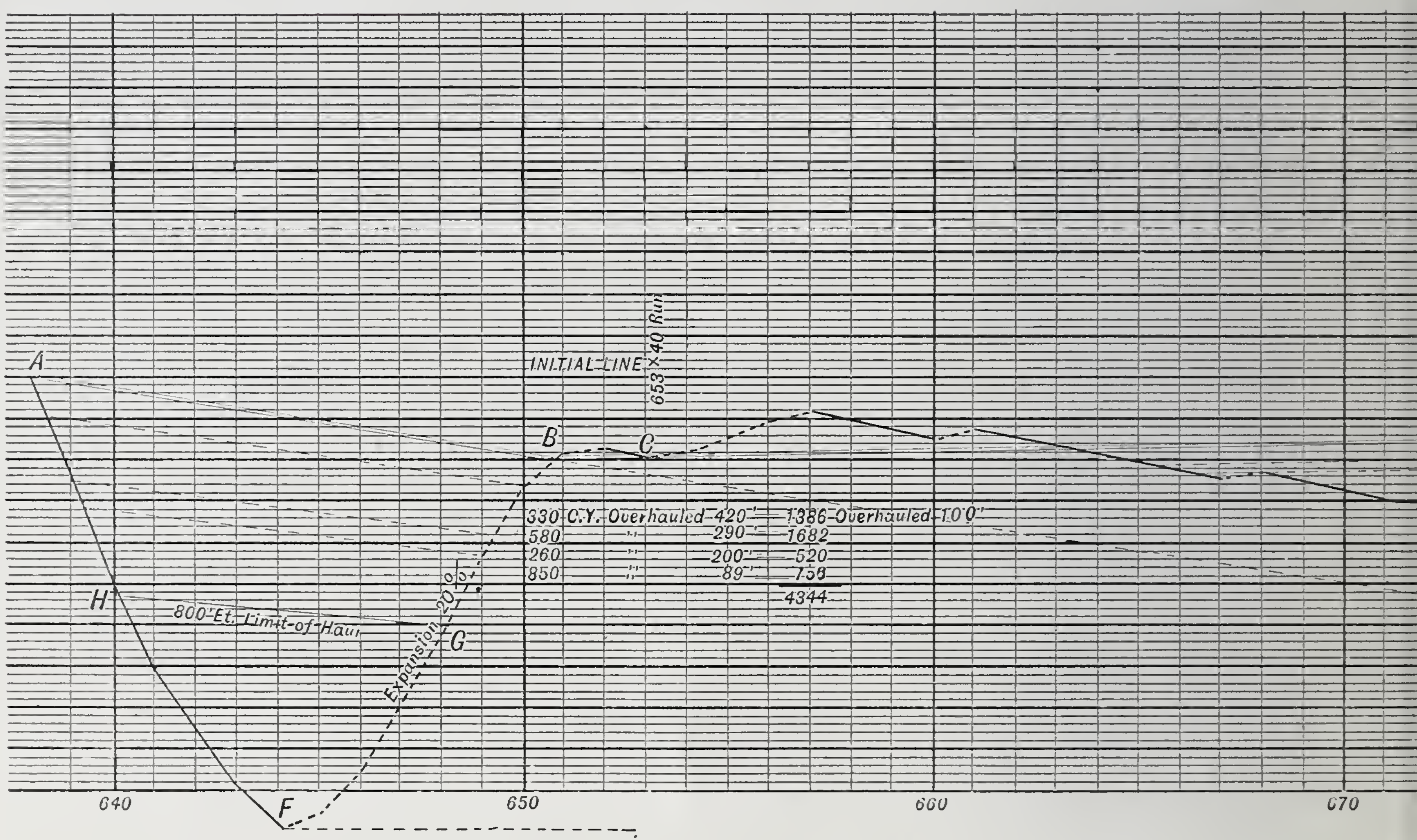
Station.	Quantities.		Cost.	Ordinates.	
	Cut.	Fill.		+	—
639	....	1,298	....	....	1,298
640	....	1,196	....	....	2,494
1	....	1,041	....	....	3,535
2	....	789	....	....	4,324
3	....	685	....	....	5,009
4	....	455	....	....	5,464
5	309	141	141	....	5,296
6	478	....	....	....	4,818
7	758	....	....	....	4,060
8	947	....	....	....	3,113
9	966	....	....	....	2,147
650	838	....	....	....	1,309
1	372	....	....	....	937
2	75	....	....	....	862
3	126	227	126	....	963
4	67	22	22	....	918
5	166	....	....	....	752
6	199	....	....	....	553
7	139	....	....	....	414
8	22	165	22	....	557
9	51	135	51	....	641
660	120	181	120	....	702
1	322	274	274	....	654
2	233	285	233	....	706
3	78	185	78	....	813
4	74	185	74	....	924
5	53	163	53	....	1,034
6	65	141	65	....	1,110
7	66	167	66	....	1,211
8	54	18	18	....	1,175
9	....	96	....	....	1,271
670	....	116	....	....	1,387
1	6	89	6	....	1,470
2	24	63	24	....	1,509
3	32	43	32	....	1,520
4	7	65	7	....	1,578
5	23	93	23	....	1,648
6	....	263	....	....	1,911
7	....	304	....	....	2,215
8	....	146	....	....	2,361
9	9	111	9	....	2,463
680	10	19	10	....	2,472
1	295	....	....	....	2,177
2	149	....	....	....	2,028
3	599	....	....	....	1,429
4	562	....	....	....	869
5	1,016	....	....	249	....
6	1,297	....	....	1,546	....
7	1,368	....	....	2,914	....
8	1,050	....	....	3,964	....
9	491	....	....	4,455	....
690	66	101	66	4,420	....
1	....	547	....	3,873	....
2	....	573	....	3,300	....
3	....	554	....	2,746	....
4	15	1,637	15	1,124	....
5	....	1,131	....	....	7
6	....	563	....	....	570
7	....	440	....	....	1,010
8	....	323	....	....	1,333
9	....	162	....	....	1,495
700	55	158	50	....	1,598
1	82	137	82	....	1,653
2	28	286	28	....	1,911
3	127	501	127	....	2,285
4	929	123	123	....	1,479
5	1,979	....	....	500	....
6	1,919	....	....	2,419	....
7	910	203	....	3,126	....
8	363	218	....	3,271	....
9	....	243	....	3,028	....
710	....	102	....	2,926	....
1	....	....	....	....	....
....	19,989	17,063	....	....	....
		2,926			

flexure of either red or blue lines, we have a series of intercepts on these lines, paired red and blue, the vertical projections of which represent equivalent cuts and fills, and the horizontal distances between their centers of gravity, less the haul, is the overhaul. To tabulate these and get equivalent quantities hauled 100 feet is the work of a minute. The shrinkage is shown by the relation between the vertical projections of $A\ F$ and $B\ F$, which may be obtained either numerically or graphically. In this case it is negative, or expansion of 20 per cent. The balancing line is continued through 684 + 20, the dividing point between backward and forward hauls in the cut, and through 695 + 50, a similar point in the fill. The last controlling point for the balancing line is the end of the profile. We have now the last two sections of the balancing line, $L\ M$ and $N\ O$, separated by a vertical distance representing 3,200 cubic yards of cut. This cut was used in widening the bank between 696 and 710. If the embankment had been measured after the widening, the red line would have been steeper and would have matched the blue. As it is the spoil bank is indicated by connecting the parts of the balancing line with an inclined line $M\ N$. Borrow Pits are indicated in a similar manner.

The application of the Profile Quantities, which we have been considering, is to work which has already been executed, where all we wanted to get was overhaul and shrinkage. By far its most important use, is in new work, for the distribution of material and for determining the average haul. The balancing line for new work is established like the grade line on the ordinary profile. Its position will be theoretically correct, when the aggregate work represented by the quantities above is equal to the aggregate work represented by the quantities below it. While all the points on this line are not determined, as in work already executed, still there will be found in nearly every piece of work, some particulars approximately or even exactly locating some points on this line. In locating the balancing line between these determined points, the engineer will find scope for the exercise of his theories and that combination of qualities which results in what is called good judgment. When the balancing line is drawn, we see the average haul at a glance. If the bulk of the quantities is moved only a short distance, it will be more economical to ask for bids with a short haul, say 500 or 600 feet; but if the quantities are largely to be moved to great distances it will be more economical to ask for bids on the basis of long haul, say 1,000 or 1,200 feet. We just read off the distribution of material. For example, on the profile we have worked out:

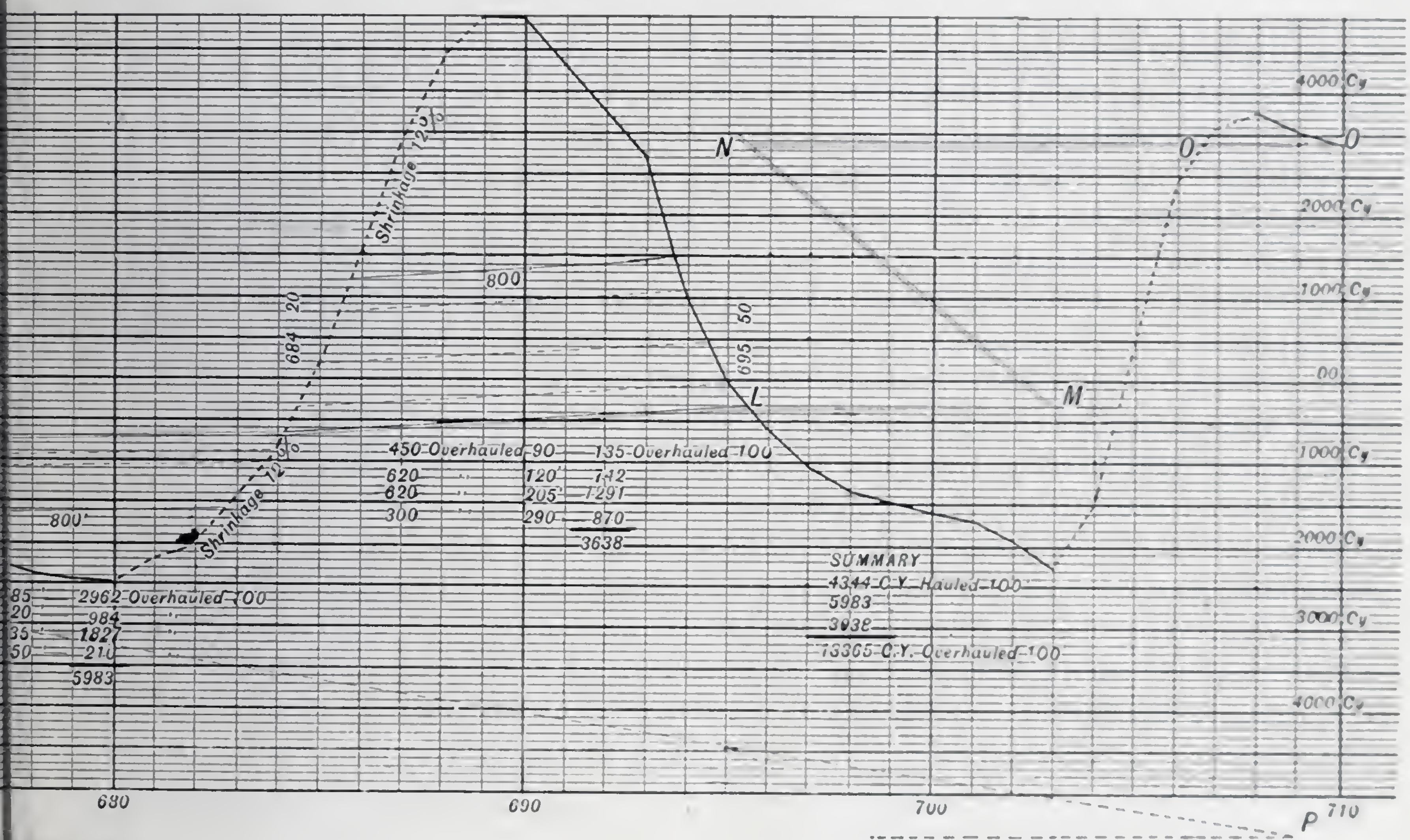
Cut from station 644 to 650 + 80, haul west, deposit between stations 638—644.

Cut from station 650 + 80 to 652, haul east, deposit between stations 652—653.


A
INITIAL LINE
653 × 40 Run
B
C
330 C.Y. Overhauled 420' — 1386 Overhauled 100'
580 " 290' — 1682
260 " 200' — 520
850 " 89' — 756
4344
H
800 Ft. Limit of Haul
G
Expansion 20%
F
640
650
660
670

PR

UANTITIES.



NTITIES

Cut from station 653 to 657, haul east, deposit between stations 657—663, etc.

The distribution of material is perhaps the most important part of earthwork. Unless the men who are doing the work are started right and constantly watched, they will put it in the wrong place. While in some cases the way in which the material should be distributed is evident without the use of the profile quantities, in every extensive piece of work, there are places where it is undetermined, and the engineer need not be surprised, if, as has happened with the writer, he finds that the profile of quantities, with its logic of facts, reverses his first impressions.

It takes more time and labor to make the profile of quantities than to work out or "judge out," as the case may be, with pencil and ordinary profile, the things which the engineer must know about the quantities with which he is dealing. But there is something in its favor. It gives a complete record of the work, which may be very important for future, as it is for present use. Distributing the material in the most economical manner, it is of advantage to the company which is having the work done, because its work is done for the least cost, and also for the contractor, who is able to do the work with the least expenditure of energy, and to the engineer it gives the satisfaction of knowing he is right.

A FROGLESS MAIN TRACK.

[A paper read before the Engineers' Society of Western Pennsylvania, Pittsburgh, by C. B. PRICE, October, 1885.]

It is believed that a stationary or permanent frog, planted in the main track of a railroad, and there subjected hourly to the wear of hundreds of wheels passing along the main track and over it at a speed of from fifteen to sixty miles per hour, none of which wheels have any legitimate business with the frog, while the actual use of such a frog is probably limited to a very few pairs of wheels per day passing through it at a low rate of speed in going into or out of the siding is an unnecessary evil in our current railway practice, and that the desirability of its abolition increases annually in the direct ratio of the increasing speed of passenger trains and increasing weight of freight trains.

When, for instance, you are making a journey from New York to Chicago upon a limited express, your train, having the right of the road, finds a clear track before it. The frogs leading into the various sidings along the road are not for the use of this fast express train. Not once is it required to take siding. Yet, in a long journey thousands of frogs must be passed over by every wheel in your train, the frogs being themselves exposed to purely unnecessary wear, and inflicting, in return, upon the locomotive machinery, driving tire, and car trucks an even greater amount of shock and injury. And this is true in somewhat less degree of all other passenger trains. Occasionally, such trains take siding, and in so doing make legitimate use of a frog; but for once that they thus find a frog useful to them, there are hundreds of times when it is but an impediment.

When we consider the greater volume of freight than of passenger traffic, and that, out of an enormous number of freight wheels passing over a given frog in 24 hours, an exceedingly small proportion of them may need to enter the siding for which the only use of this particular frog is to afford ingress and egress, it is easy to comprehend that even at the lower speed of freight trains there must be relatively a greater unnecessary wear of frogs on the part of freight wheels than of passenger wheels, and that all this friction is attended with small recompense in the way of useful service.

An axle slightly bent—a flange a little too sharp—a driving tire worn a little too hollow—each defect almost too slight to meet the condemning glance of a vigilant inspector—may pass safely over hundreds of miles of steel rail, but contact with a frog may, and often does, cause an accident that would not elsewhere occur.

A permanent frog with its entering angles like bootjacks has caught many a foot in its deadly grasp, and brought many an honest fellow face to face with death in one of its most horrible forms.

Consequently, the question of the real necessity of fixed and permanent frogs in the main track is one well worthy of consideration. Can they be dispensed with, and in their place can there be used a movable equivalent, which will allow passage for wheels in or out of the siding when it is necessary to use the siding, while at other times this frog substitute shall stand clear of the main track, free from exposure to unnecessary wear, and permitting trains to pass along at high speed over an unbroken rail?

It is believed that this question can be properly answered in the affirmative, and it is proposed to state to you what has been, and is now being, done toward this end.

The history of movable frogs is not a recent one. Indeed, I am informed and believe that a crude form of movable frog antedates the fixed frog, even on steam railways. In the mine tramways of Great Britain and this country, it is customary to run loaded or empty mine wagons on to, or out of, turnouts by throwing a movable siding rail over the main rail; and when steam railroading commenced the practice of the mine tramway in this respect was to some extent followed. A gentleman formerly resident in this city who has occupied various high railway positions, told me not long ago that he could remember when such was the practice on the Camden & Amboy railroad.

I do not understand that these early substitutes for frogs were interlocked with the switch, but that each was thrown separately; thus it was necessary to throw two levers, situated at different points, to enter or to leave a siding. From such crude devices as these, however, the permanent frog was a happy relief; and its use rapidly became general.

In those days of butt switches and strap rails, nobody thought seriously of the additional jar caused by wheels passing across the throat of an open frog; and a generation passed by before the spring-rail frog, with its greater smoothness and comfort, came into vogue on the first-class lines, while on a very large proportion of the railways of this country, the jarring "open-throats" or "straight frogs" are still exacting their tribute of discomfort from every passenger and wear from every wheel.

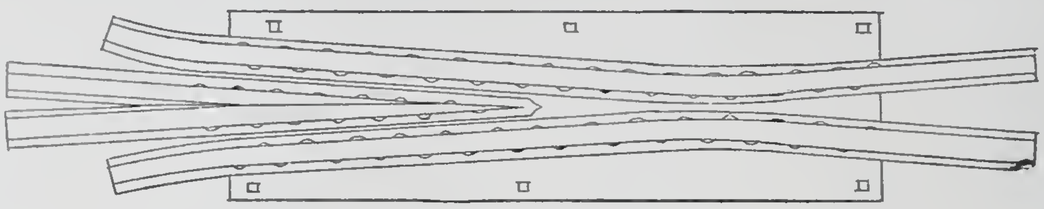


Fig. 1.—Straight or Open Throat Frog.

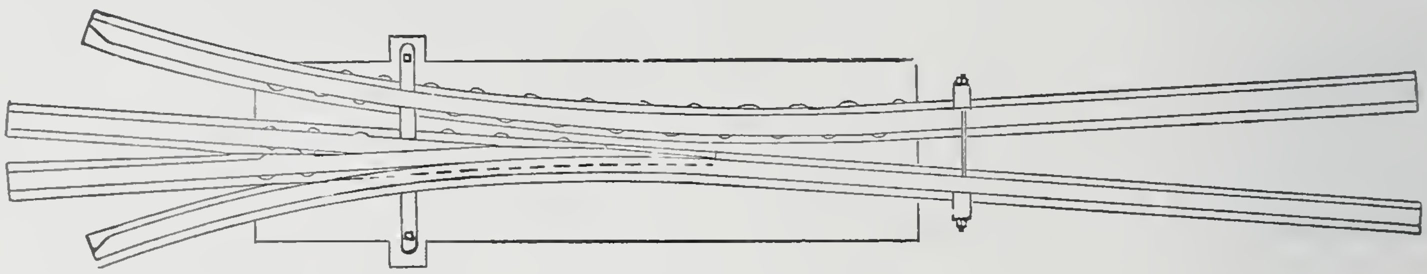


Fig. 2.—Spring Rail Frog.

In 1849 a patent was granted to William Dutton for a removable bar or rail connected by a long rod with the switch rail, and arranged to be thrown across the main track at the point usually occupied by the frog. I cannot find that this device was ever put into use, but a few years before the war a switch sometimes called "the telegraph switch" and at other times "the Dooley switch" was introduced on several southern roads. In this switch, a pivot rail in the main track at the point usually occupied by the frog and connected with the switch by a rod, took the place of the frog. When the switch was thrown for the siding, the pivot rail was made to turn on its centre, so that its free ends came in contact with the lead rail and siding rail respectively, and afforded a passage into the siding for the wheels of the train.

Many of our northern railroad men who entered the Government Military railroad service during the war, obtained their first sight of a frogless main track on one of those Virginia roads; and they were all ready to vote against it, as a remedy for the frog evil that was worse than the disease; because it left two open joints in close contiguity in the main track, which jarred more than any frog, and because in warm weather with an expanded rail, it was a matter of extreme difficulty to operate both the switch and pivot-rail. Bars and hammers were often necessary to drive the ends of the pivot rails into line with the adjacent fixed rails. The "telegraph switch" does not appear to have made any headway in the north, and to have soon had its day at the south, although not long ago I heard of several of them on a branch road in Alabama.

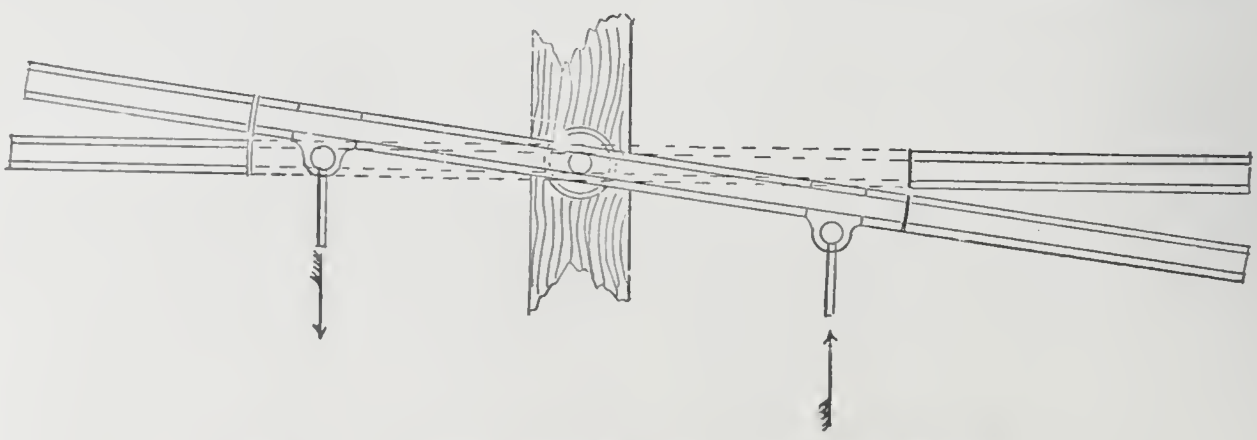


Fig. 3.—"Telegraph" or Dooley Switch.

Between 1860 and 1870 the principal improvement in the track appliances of railroads in this country was in the substitution of safety switches for the barbarous stub switch that had been endured previously. The English point switch,

improved by Lorenz and by Armstrong, and the Wharton switch, a distinctively American invention, became the successors of the old fashioned stub. The Wharton switch secured an absolutely unbroken continuity of rail at the switch, and the point switch gave practically although not the same result.

offering to supplant the fixed frog now known to railway practice.

In the first place, the frog-substitute must be as firm and stable as the frog itself. Although as compared with the present unnecessary use of the frog, its necessary use may not be greater than is 1 to 100, yet, when it is to be used, it

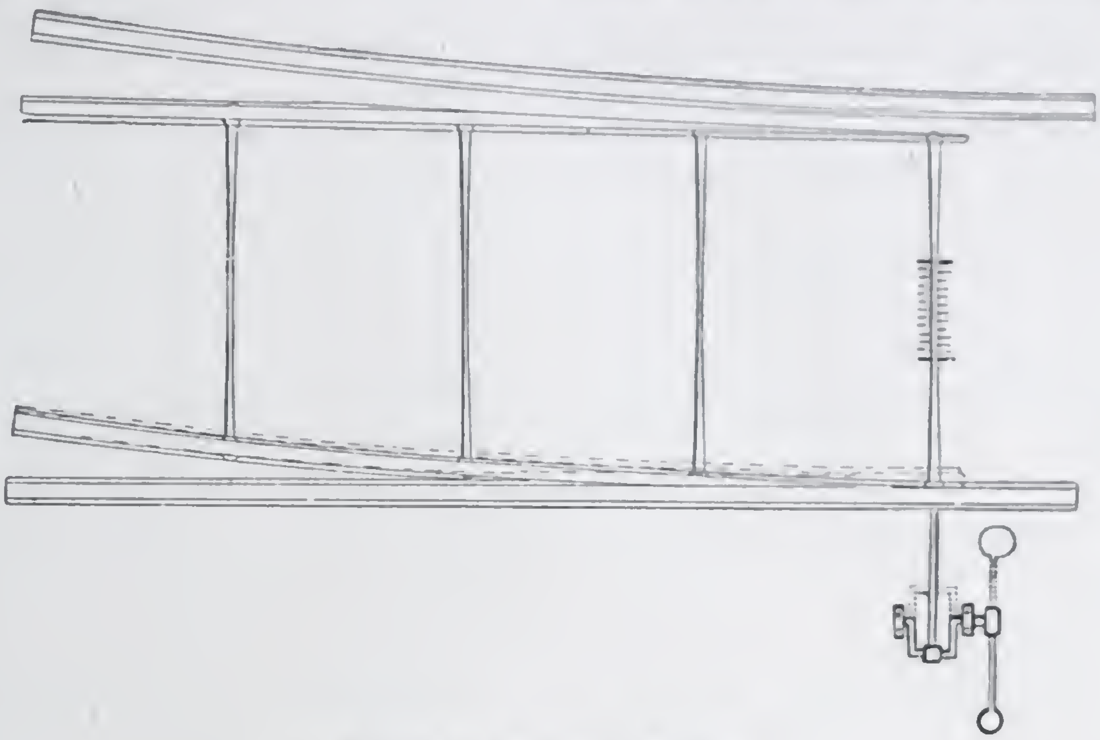


FIG. 4.—POINT SWITCH.

Both of them secured safety to trains running over the heel of the switch in the event of its having been carelessly set wrong (or, as it is called, "left set for the siding") and with this change came a highly satisfactory lessening of the dangers of travel.

A continuous rail at the switch having thus been secured, the break or gap at the frog remained the only one to close. Within the last twenty years, quite a number of patents have

may be used by the heaviest engines and cars; and who can say at what speed? If the present frog, fixed in the track as firmly as any part of it, has any merit that is beyond question, surely it is in this quality of offering a solid bridge for traffic coming from one track to another; and any movable substitute, no matter how great may be its advantages in other respects, will certainly fail of approval if it retain not this excellent quality of that which it seeks to supplant.

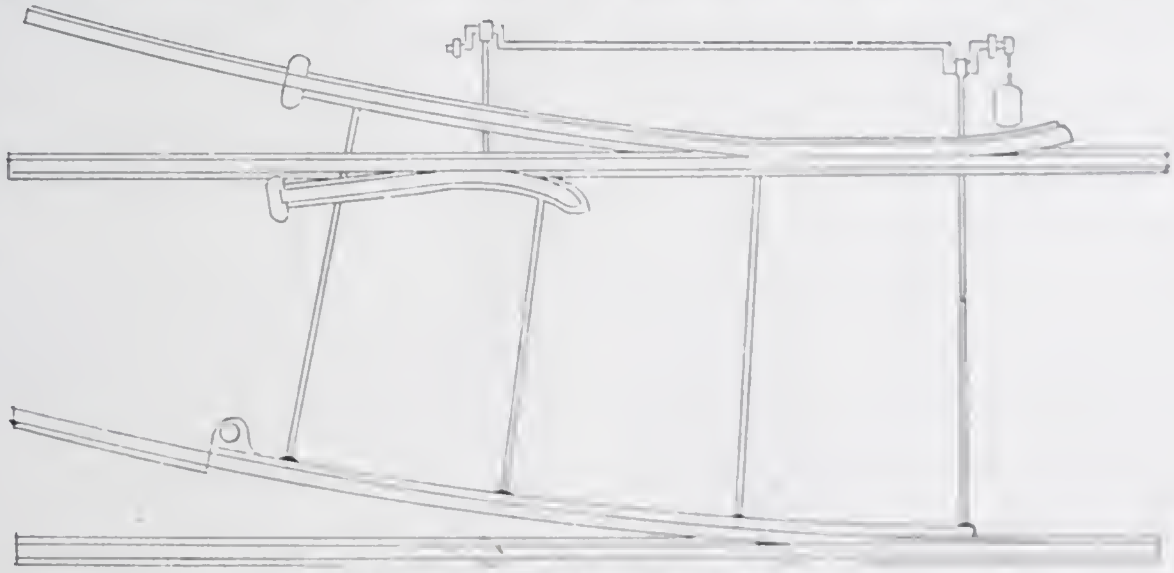


FIG. 5.—WHARTON SWITCH.

been granted for devices intended to take the place of frogs, but the manufacture of frogs has not ceased, nor has any feasible frog-substitute come into general use, or even been heard of by railroad men in general. Why is this the case?

It may be well to consider just at this point the conditions that must be met by any device

And again, after all that has been done in the last twenty years to secure safety switches which cannot derail a train even if, once in a while, they are left misplaced—surely railway managers would not, while they retained the possession of their faculties, favor for a moment a movable frog, which in the event of the same

degree of carelessness in their operating force, would by being left superimposed upon the rail at the point usually occupied by the frog, derail a train at that point and thus render nugatory the safety feature of the switch with which it might be interlocked. In other words, the movable frog which is to drive the stationary frog to the same shades of obscurity in which the old stub switch now reposes, must possess the same feature of "safety under careless operation" which would be claimed for the Wharton, the Lorenz, the Armstrong and other successful rivals of the stub switch.

Then it must be possible to perform over the frog-substitute the various every-day operations of railroading. Among these are "flying switches" (or as it is sometimes called "swinging" or "dropping") and "roping." The "flying switch" is strictly forbidden in many quarters. In several States there are statutory provisions against it. On many railroads there are general rules and special orders against it.

If swinging is not to be practiced, then under similar conditions of work, "roping" must be. This, as every one knows, is done by stopping the train just in front of the switch, attaching the switch rope usually carried on the engine (and generally from 12 to 15 feet in length) to the drawhead of the tender at one end and to the drawhead of the car at the other end; then throwing the switch between the engine and car whereupon the engine proceeds along the main track, while the car guided by the switch and pulled by the rope, enters the siding.

Now if the frog-substitute be so interlocked with the switch and of such a character that when thrown for the main track, to do its work in lieu of the frog, it presents an insuperable obstacle to an engine passing along the main track, then it is objectionable and cannot fill the place which it is designed to take.

A final and important point upon which a frog-substitute must expect to stand judgment is that of cost. It is doubtless true that a cost not exceeding two to four times that of the present low price of stationary frogs would not seem excessive to many managers—certainly not to managers of those great trunk lines upon which the immense unnecessary wear of their frogs shortens their life to three or four years; but if the proffered substitute should come too high in its first cost, or if it be of such complicated and cumbrous mechanism as to make it reasonably certain to require frequent and costly repairs, then again it would not meet the requirements of the case.

These then it would seem are the requirements of a movable frog, necessary to secure preference over the present form of fixed frog; first, absolute safety in ordinary operation, involving the use of the heaviest rolling stock now existing or likely to exist; second, safety to trains passing along the main track, in the event of carelessness attending operation; third, no addition to present difficulties in performing work at sidings; and fourth, relative economy as compared with fixed frogs, if not in first cost at least in cost of maintenance during a term of years.

Now, when we look at the devices contained in the records of the patent office (for it is not possible to learn much about the majority of them from any other source of information), it becomes apparent that a large proportion of those who have taken up the subject, even in these later years, have done so in about the way that Dutton and Dooley, way back in the 50's did. It is surprising to note the very slight variations between many of them. Most of them do not contain the slightest provision for safety in the event of the switch, with which the frog is interlocked, having been carelessly left misplaced; and the noting of this fact leads to the supposition that most of those who have been thinking upon the subject have not been practically engaged in railroading, for in these days of safety switches no railroad man would think of placing in the track a device that would annul their advantages.

In various devices, however, a safety feature is introduced; and the favorite plan seems to be either to so construct the movable bars or rails crossing the main track in lieu of the frog that if carelessly left in that position they will be pushed aside by the first pair of wheels coming along the main track; or else to so arrange a lever in the track behind the movable rails and connected with the latter, that upon this lever being struck by the wheels it will throw the movable rails out of position and clear the track for the train.

A device patented by Wm. H. Stowell, of Baltimore, Maryland, August 17, 1868, is the earliest exemplification of the first described plan; and a patent granted quite recently to Mr. Seth Curlin, of Covington, Tennessee, is the best illustration of the latter one. Covering various shades of difference in these two principles are quite a number of patents. Indeed, the existence of so many would naturally surprise a person, who having heard little or nothing of the subject before, might reasonably expect upon entering the field, to find it a virgin one. The query at once arises, why have they failed to gain introduction or even a degree of notoriety?

It is believed that there are at least three serious objections to these various forms of movable frogs to which for convenient description,

the general term of "Kickaway frogs" may be applied:

First, that it might not always be safe to trust to the rapid and sure action of the safety levers or other appliances upon which the clearance of the main track, and the safety of a train at high speed depends, and that this might be particularly the case when snow and ice had clogged together and bound down the various parts.

Second, that the operation of roping is apparently obstructed, for when the engine coming up the main track "Kicks away" the frog, and by the same action closes the switch with which it is interlocked, how are the cars to go into the siding? It is true that it would not be impossible to overcome this fault by special mechanism, but it would add to the complication and cost.

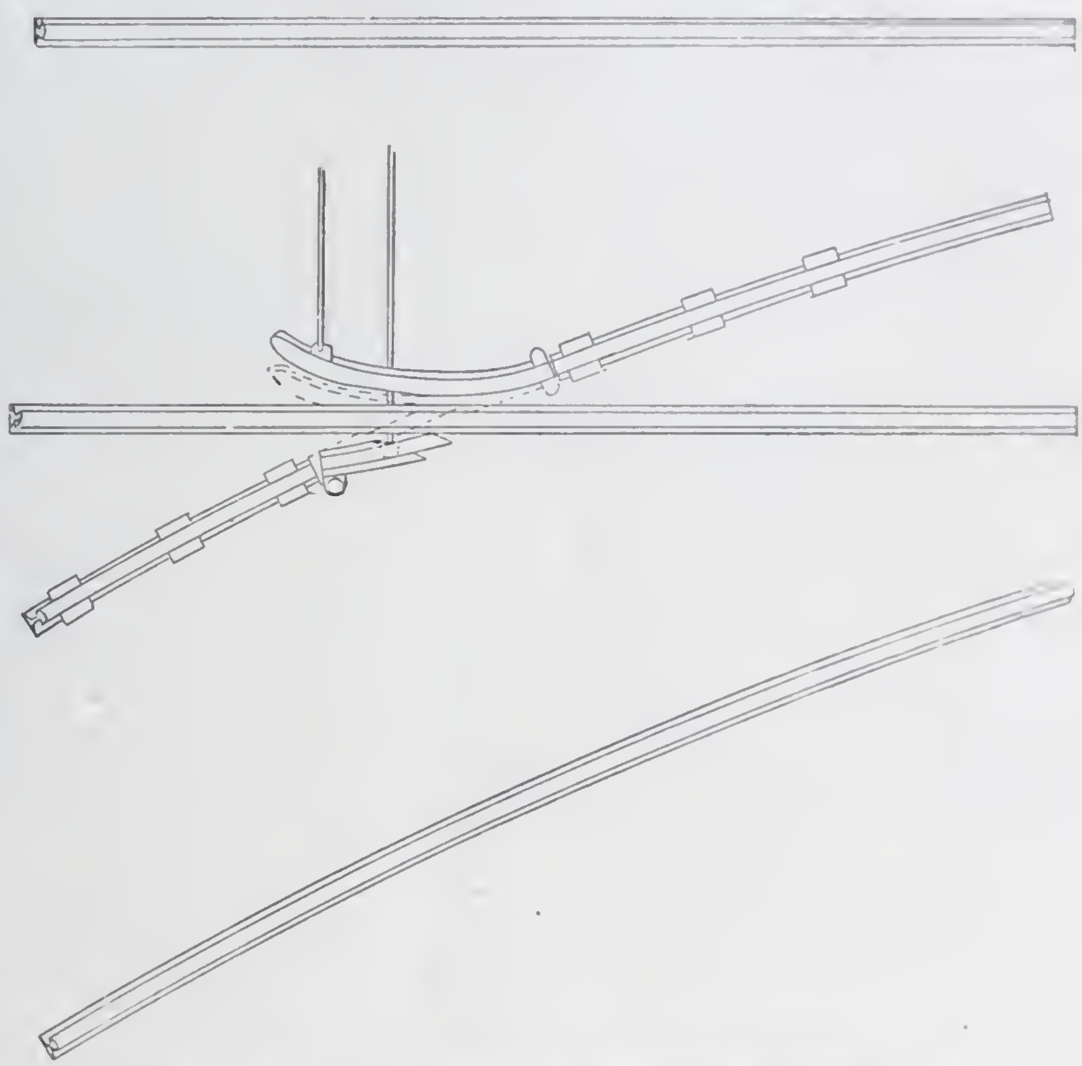


FIG. 6.—POLLOCK FROGLESS SWITCH.

Third, (and doubtless the most important reason) it would appear that any movable frog so arranged and so loosely held in position, that it can when necessary be thrust aside by the action of the wheels, must, in the nature of things lack the rigidity—the undeviating firmness of position—which it has been previously stated is considered the pre-requisite of a frog for the ordinary purpose of carrying wheels from one track to another. I consider that it is an absolute requirement to give a movable frog the same property of rigidity, when once thrown into position, that a fixed frog can have; and that any arrangement designed for safety—(which arrangement may only come into play upon an average once in ten years, or may never be used at all) that militates against the absolute fixedness of the frog point for its daily legitimate use, is, although well-intentioned, a serious impairment of the efficiency of the frog substitute.

In this matter I speak from experience, having some time ago encountered the difficulty of undue vibration, in the first experiments tried on the Allegheny Valley Railroad with movable frogs; and it was not until this lateral vibration (even though in our case it was slight) had been entirely removed, that I began to think that it was possible to dispense with fixed frogs in the main track.

After diligent inquiry I can only hear of two actual tests of "Kickaway" frogs having as yet been made. A frogless switch upon the principle first described, and covered by the patents of Wm. H. Stowell, Darius Pollock and Wallack & Rohrer was placed in use in the Richmond, Ind., yard of the Chicago, St. Louis & Pittsburgh railroad about a year or two ago. I visited that point a short time ago in the hope of seeing it in practical operation, but was disappointed, as it had been taken up and sent to the Indianapolis shop for repairs; but I met Mr. M. W. Mansfield, engineer of maintenance of way of that division under whose direction it had been put into use; and he courteously gave me a great deal of information with regard to it. It had

not been in use in the main track, but to connect a short coal siding with a somewhat longer warehouse siding, in which position I was informed that it had fully answered the intended purpose of dispensing with the frog. I regretted to have not had the chance to see it tested, as this would have either confirmed, or dispelled, my views with regard to the instability and other defects of this kind of a frog substitute, under shifting of ordinarily heavy character.

I was recently visited by Mr. Curlin, the inventor of the other device upon substantially the same principle that has just been alluded to, and was informed by him that a siding on the Chesapeake & Ohio railroad near Memphis, Tennessee, had been equipped with a frogless switch of his pattern for some time past and that it was giving good satisfaction. In that latitude it had not of course been subjected to the action of cold weather, nor had it received the severe tests which he was informed had been applied to the A. V. R. R. switches, but he thought it considerably cheaper than mine, and claimed that it would answer all that was required of it at a great many points where fixed frogs are now in use. Mr. Curlin's safety device is precisely similar to the safety device used for the Wharton switch, but the rails used by him are of unique pattern, and cause doubts to arise as to their durability. Like everything else, however, it needs to be thoroughly tested, and if it can stand the impact and momentum of heavy trains as well as any other form of frog substitute, Mr. Curlin will doubtless meet with the measure of success which I should be pleased to see fall to his share.

I now propose to speak of what we have been doing upon the Allegheny Valley Railroad, and having already treated the general subject at considerable length, I propose to be as brief as possible in describing our own frogless switch. The subject came to my attention several years ago, and after contriving many forms or variations of the "Kickaway" frog (not knowing at that time that the ground had been previously gone over so often by Stowell, Pollock, Williams any many others) and after dismissing all of these devices as impracticable for the reasons already stated, I finally decided to so construct my frog substitute that in the event of the switch being carelessly left wrong it would be possible for a train running along the main track at any speed, high or low, to mount over it, certainly with safety to itself, and probably without serious injury to the frog. The latter point —that of breakage of the frog under such circumstances—I cared little for, if only there were no derailment of the train; for it was not to be expected that such occurrences would be common on any decently regulated railroad, and the occasional cost of replacing a frog broken under maltreatment of this kind would be a small matter.

The model, and the photographic views now

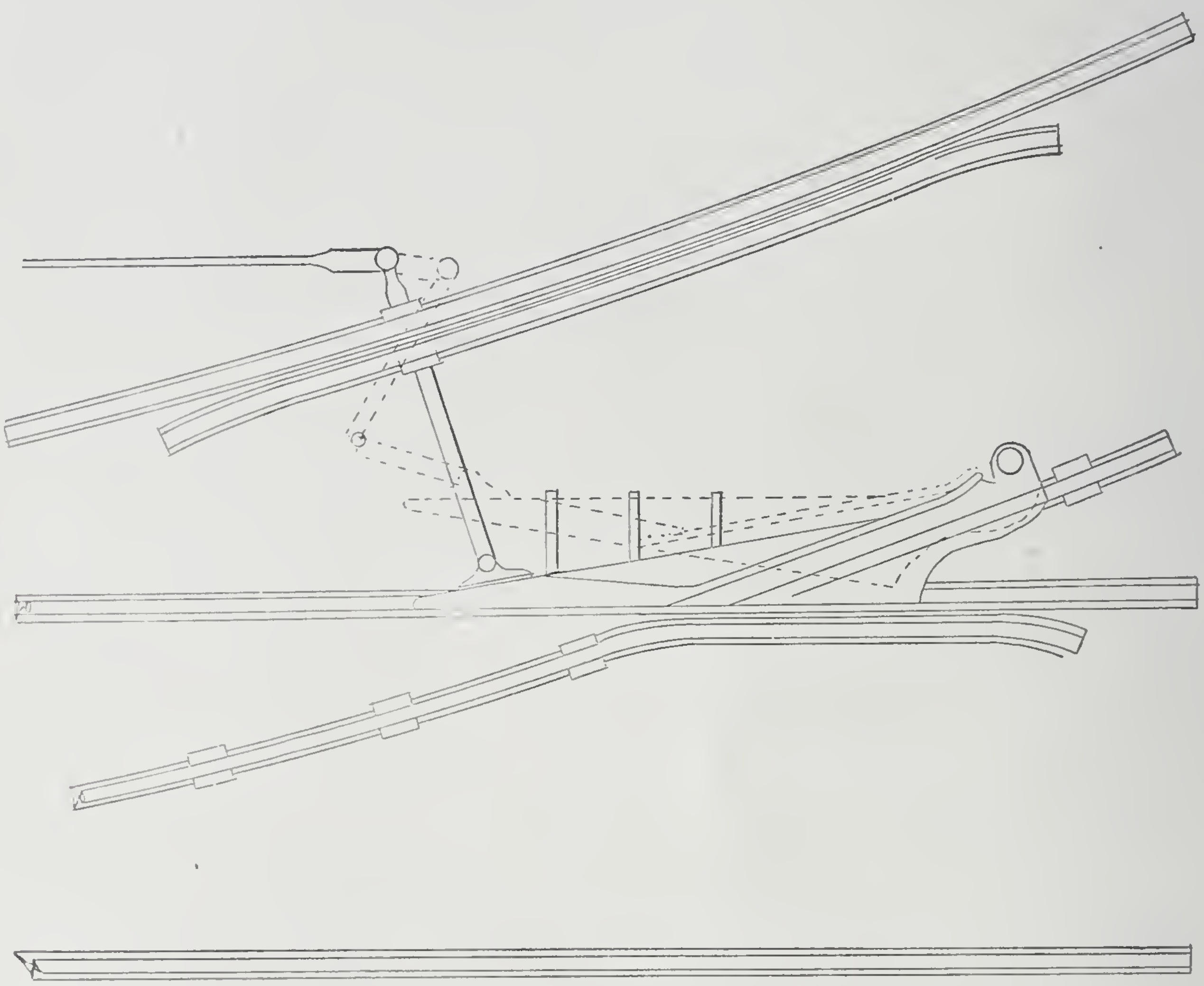


FIG. 7.--PRICE FROGLESS SWITCH.

before you, will show the mechanical results following this line of thought. You will observe that the movable frog substitute is of peculiar construction, resembling in appearance an ordinary frog with the inside wing rails cut off, and having at front and rear of the point, sloping projections arranged to fit over the top of the rail, when the switch and frog are thrown for the siding. Motion is communicated to the frog simultaneously with the throw of the switch, by means of a connecting rod or rods of necessary length having either a reciprocating or a rotary travel. The movable frog slides on chairs which carry the bottom of the frog on a level with the top of the main rail, which is, of course, unbroken and continuous at the point where it is usually cut for the admission of the ordinary frog. The height of the frog point is about two inches above the top of the main rail, and is on the level or thereabouts, of the end of the lead rail, which is raised on chairs or plates, the object of this elevation being of course to carry the deepest wheel flanges over the top of the main rail without touching the latter. We gain a sensible advantage by this elevation of the lead rail and frog bar in an important respect, viz.:—that of placing in the outside of the curve leading into the siding the amount of elevation called for by the curve, and which when there is a fixed frog in the siding, it is impossible to give, because the frog always holds you down rigidly to the level of the main track. With the help of this elevation, we find it possible to enter or run out of a siding with a steadier motion than over any fixed frog, plainly the result of the conformity with physical laws which a frog bar lying on the rail permits, and which a fixed frog held in the main track denies.

The sloping projections at front and rear of the frog point, heretofore referred to, decline from the height of two inches at the frog point to a thickness of say one-quarter inch at their extremities, thus forming inclined planes adapted to the guidance of wheel treads when the latter are compelled to surmount the frog. But this is not the only use of these safety wings. It is rarely that they will come into use for the carriage of wheel treads over them, unless it be in the operation of roping which will be referred to hereafter. Their everyday value, and it may therefore be said their principal value consists in the length of bearing upon the main rail which they give to the movable frog-bar, and consequently the increased stability of the latter under the pressure and momentum of heavy trains pulling in and out of the siding. Additional rigidity is also given to the frog-bar by the peculiar mechanism by which motion is transmitted to it from the main operating rod, which as before stated may be either rotary or reciprocating, but in either case is made to actuate toggle levers fulcrumed between the main siding rail and the adjacent guard rail, these toggles being capable of exerting the strongest possible leverage, quick motioned at first and slow and powerful at the finish, to drive the frog-bar to position, and hold it there as in a vise.

The first frogless switches we put into use did not have the benefit of this strong leverage, but were operated by ordinary bell crank levers, and although the long safety wings gave us an excellent longitudinal bearing, I soon found out that to bear the stress and strain of all kinds of wheels and weights, we must cure the tendency toward excessive lateral vibration which bell crank levers permitted, and which in the nature of things, would not lessen with time, but would grow worse as the wear of parts, and resulting lost motion in them increased. Then the present throw by toggle levers fulcrumed in the track itself (by taking advantage of the juxtaposition of the guard rail and siding rail) was adopted; and this remedied the difficulty. We now find that the movable frog-bar, when once thrown, is held to place as firmly as the best anchored permanent frog, and we consider that the first condition, viz.:—stability under ordinary use—has been fulfilled.

Now comes the important question of safety under careless operation. It is a comparatively rare thing nowadays to leave a switch wrong on any railroad presenting even ordinary conditions of discipline. The rule is almost an invariable one to hold conductors personally responsible for main track switches, and a rigid enforcement of this rule secures to them the best attention of a class of men who have gained their position by carefulness and intelligence, and who cannot afford to be negligent. Then again the general use of targets makes carelessness of this kind conspicuous, and the air brake makes it possible for engineers to stop quickly upon sight of a red target. For all these reasons, "running through" misplaced switches is getting to be a rarer occurrence every year. Nevertheless, it sometimes occurs and will always be liable to occur. Like a revolver in Texas the safety provision "may never be wanted in a life time; but when it is wanted, then, stranger, it is wanted blamed bad."

At first sight, it hardly seems reasonable to claim that these thin safety wings can perform the function of carrying the wheels of a passenger train at high speed, or of a heavy freight train, up to and over the point of the superimposed frog-bar; but let it be remembered that they are shaped to the top of the rail, rest smoothly upon it, and have the support of the

entire rail section beneath them. Seeing is believing. I did not believe myself until after many tests had been made that they would perform such efficient service without breakage or injury to themselves. Of one thing I was always quite sure, viz.: that with the usual guard rail on one side of the main track opposite the frog, and the elevated lead rail practically constituting a guard rail on the other side, thus making a double throat for flanges, there was no chance of derailment, even though the thin edges of the safety wings of the frog might break. Such breakage would simply uncover the main rail beneath, and should not have condemned the frog as long as derailment was prevented.

I may say, however, that scores of tests made since last February, in which the frog-bar, purposely left misplaced upon the track has been passed over by engines at forty miles per hour, and by freight trains of forty-five cars at fifteen miles per hour, have failed as yet to cause breakage and that no more than an ordinary amount of wear has been apparent under this persistent maltreatment.

Now as to third condition: It will be obvious how roping can be performed. After the engine clears the switch and the rope is fastened on, the switch is turned between the engine and car and by the same motion the frog-bar is, of course, thrown upon the main track in front of the engine, which finds no difficulty, however, in surmounting the obstruction by means of the safety wings, while the car or cars, guided by the switch and pulled by the rope, enter the siding through the throat of the frog.

The fourth consideration—that of expense—may be disposed of by the statement that the first cost of our movable frog is within the prescribed limits. If we consider for a moment the question of durability, it will be seen that the wear upon the movable frog must be so infinitely less than upon a fixed frog that its duration of life as compared with the life of the latter, can only be roughly approximated. I will not attempt to say at present how much longer the average life of a movable frog is likely to be. It sometimes seems to me, however, that it will be more likely to rust out than to wear out. Another point that commends it to the transportation department is that in the event of a breakage of the frog bar or of its deterioration through wear, the main track would not be affected thereby. It would not be necessary to send for section men or to flag and hold trains while the broken frog was being replaced. The worst that could result, would be the temporary non-use of the siding. It may be presumed that upon a general introduction of frogless switches, each track section would be supplied with one or more extra frog bars, so that in the event of one giving out, it could be quickly replaced from the nearest tool house, using a hand car for moving it to the desired point of use, and that even the interruption in the use of the siding would be a matter of a few hours only.

We have two frogless switches in use in our 43d street yard, Pittsburgh, and are about to place another in position in that yard; there is one at Verona and another about to go in at that point; and there is one in at DuBois on low grade division. They have given satisfactory service. All of the above switches are point switches of the Lorenz pattern except that one at Verona at which we are about to make a change, which is a Wharton switch. The peculiar construction of the Wharton switch requires some changes in the connecting rods and safety appliances which I will not now stop to describe, but the safety wings and the toggle leverage of the movable frog which we have heard described, will not be departed from.

In conclusion, I beg to say that it is not presumptuously supposed that our frogless switch has all the excellences and none of the deficiencies, or that a frogless switch upon any other principle might not meet the approval of its users. While not an untrodden field, it is a new and large one; and doubtless the inventive talent of the country will make it fruitful in large measure ere long. I am happy to have had the opportunity of directing the attention of our society to the work that has already been done in this direction, and to the room for further effort that remains to the end that the fixed frog twin relic, with the stub switch of a former era of mechanical barbarism may be swept from the main track.

DISCUSSION.

Mr. Davison:—I would like to ask Mr. Price, of what material this frog is manufactured.

Mr. Price:—My first intention was to construct it of steel rails, but I commenced to use a steel casting, and have been so well satisfied with its use so far that I have not as yet made any change in that direction. However, I do not really expect that I have reached a permanent shape yet.

Mr. Rodd:—I have been very much interested in this paper, as have all our members, for it is certainly one of the best papers on maintenance

of way I have ever heard read on the subject. And the subject is one of so much importance that I hope there will be a discussion. I do not know that there are a great many railroad men present, but I think a good many of them could say something about it—their experience with frogs and switches in general.

There is no doubt that a great many accidents, and very serious accidents, have occurred from the simple fact of the frogs along a track being in bad order, and if any device of this kind will, in any measure, remedy that, it will be appreciated and be successful. It will be one of the best things that was ever introduced on a railroad in a track.

I would like to ask Mr. Price how long these frogs have been in use on the Allegheny Valley railroad?

Mr. Price:—We put the first one in use last February. I think the next one was put in in April.

Mr. Rodd:—I was also going to ask the approximate cost.

Mr. Price:—I could hardly arrive at an average as yet. The cost, however, has been steadily decreasing. The first one cost about $100, the next one $90, and the next $80. That cost includes not only the frog bar itself but the connections to the switch.

Mr. Rodd:—In that frog I presume that that point moves on the rail, the same as iron slide bars. Now, in case of a frog on the main track, it is necessary to keep that main track rail in good shape, as a part of the frog. You would not let that main track rail wear down until it would not give a bearing?

Mr. Price:—I should not like to, but that could be remedied by adzing the ties at the points where the chairs rest upon them, approximating to the wear of the main track. That is a device which might be adopted.

Mr. Rodd:—It should be given a very full and clear trial. I would be very glad to see it successful.

A Member:—You could not adz these down with the present construction. The slides rest immediately on the bottom flange.

Mr. Price:—No, sir, the slides rest on the ties. I believe the chairs in this photograph (exhibiting a photograph) consist simply of short pieces of steel rails, which were adopted because they gave exactly the right height.

Mr. Roberts:—From the reluctance of the members to speak on this subject I think that they can not find any fault with it. I think the paper was so admirably written—it seems to cover the history—and in fact we are all probably better informed about the subject than we ever knew before, that I have been sitting here trying to think of some defect in the principle. It may be possible yet that some improvement will be made, as Mr. Price suggests, in further practice. It is certainly a very admirable thing. I am astonished it has been so long in use—since last February—and not more made. I think some of our Pittsburgh companies here, officials, should look into this. I certainly should recommend them to do so.

Mr. Brown:—I presume a great deal of the apathy results from having the standard frogs and switches. I had an idea of the same kind, but I think it should be classed with some of the others you have just cited—it was of no account.

Mr. Price:—One might suppose that the severe weather of last winter would have had a great effect on this subject. There were so many broken frogs on all the railroads around here last winter that it is a little surprising that some attempts have not been made to regulate and improve the matter.

Of course last winter was an exceptionally severe one. The frost was severer and I think more continuous than during any winter within my recollection at this moment. All kinds of metal seemed full of it. We had to watch our tires very closely and to shop engines promptly for the least hollowness. I am told that others' experience was the same as ours. In fact some tell us we escaped well. We had more broken frogs last winter than ever before. Now, if frogs could be taken out of the main track altogether I know that engineers would go over the road with a great deal more confidence of getting over the road and not into the ditch.

Mr. Rodd:—I believe that a frog to the mile on the average on a pretty busy railroad is about the proper number. It certainly would be an enormous saving for the railroads to use something like this and do away with frogs. Supposing you put the frog in at the low price of $25. I do not believe a frog, an ordinary stiff frog, would last more than a year and a half. On a very busy railroad, with a great many trains per day, they possibly would not last much over a year on the average. Sometimes, I know, that they have been taken out in not over three months after being put in the track. You can see the enormous amount, at $25 per mile, on a long railroad, it would cost per year. It would be a big item in the way of expense.

Mr. Brown:—How much does the ordinary spring frog cost?

Mr. Rodd:—I think $31. Very cheap compared with what they were.

Mr. Price:—They are much cheaper now than ever before.

Mr. Rodd:—A great many accidents have happened with spring frogs. They aid the passage

of the rail over the track but at the same time many serious accidents have occurred with them.

Mr. Brown:—I am satisfied, so far as my judgment goes, that this is a very superior frog to the spring frog. The spring is liable to break and throw the frog out. The rail end is thrown out of gauge, and then there is liability of some of the wheels dropping in between the two rails.

Mr. Rodd:—A spring frog endures an enormous amount of shaking (shock) from trains running out of the siding. Every wheel strikes the end and gives it a jar, and most spring frogs are made in a way not to give them a good resistance.

Trains run out of a siding very fast, as much as 15 to 20 miles per hour, and every wheel strikes the end of the movable rail very hard, and if it is not shaped with a very easy curve so as to let the flange of the wheel strike it gently, it gives it a tremendous shock, and frequently puts it in that condition it is ready to drop out of place, from the movement of the wheels along the main track. Then a train comes along the main track, moving with much higher rate of speed, and in that way there is trouble.

Mr. Brown:—Also, in passing out of the siding, there is a tendency to loosen the wheels.

Mr. Rodd:—It is hard on the wheels too, and on the axles also.

Mr. Roberts:—I think, Mr. President, the chief merit of that model is the safety to travel on trains at high rate of speed, no danger from broken rails. Then it is not at all complicated. Its simplicity is also a high recommendation.

By a Member:—Does it take great power to throw that lever?

Mr. Price:—No, sir. I expected when we put the first one in use it might require some additional power to work it, but on the contrary I found it was rather easier than a stub switch lever and fully as easy as the Wharton switch lever.

Once you get the lever straight it almost turns itself. The stub switch lever is a direct measure of main strength, and while the weight to be moved is not very great, yet the strength seems to be used in the worst possible way for economy.

Now, I found, when we had our switch and frog coupled up and properly oiled, that the throw was really quite light. We had to get it over the center, you know, like the Wharton switch, and once that was done it threw itself.

I am free to say I was wrong in one important respect and that was in selecting the reciprocating motion for the throw instead of the rotary. One reason why I did not do that was because I was afraid a rod of the length required, unless made quite heavy, would have a great deal of torsional strain, and if made so heavy as to avoid this it would be too heavy. I believe now I was wrong, and from all I hear and am told by those who are conversant with the subject of heavy and long rods, I believe that it will throw easier than the present one.

Mr. Rodd:—How sharp is the frog—how small the angle?

Mr. Price:—We have one No. 7. All the rest are No. 9.

Mr. Davison:—Mr. Brown's experience is my own, with frogs and switches. It commenced about the same time I believe. Since that time I have continued to have a little experience with them.

And like Mr. Brown I do not see anything particularly that I can criticise in this frog. In fact, like the rest, I can see a great deal of merit in it. Mr. Price mentions that the frogs made recently are No. 9 frogs. Within the last two years, with some ordinary spring rail-frogs, No. 9, I have had a little experience in having the wheel tire of engine wheels cut the No. 9 frogs open and allow the wheels to drop in beside the track causing several light wrecks and, of course naturally enough, considerable expense.

This frog of Mr. Price's of course does away entirely with the spring rail frog, and using the same number of frogs which has been so disastrous with us several times, it would probably have been better for us if we had had it, or some such device as this.

I should say, in this connection, that the engines were not our own engines. They were engines from a foreign road and consequently we were not as much at fault as would appear. We shopped them for repairs several times to their owners but, as is generally the case, the owners did not care as much for the repairs as the parties using them.

PITTSBURGH TESTING LABORATORY SPECIFICATIONS FOR MATERIAL AND WORKMANSHIP OF STRUCTURES OF IRON AND STEEL.

[A paper read before the Engineers' Society of Pennsylvania, December 15th, 1885, by ALFRED E. HUNT, Inspecting and Metallurgical Engineer.]

WROUGHT IRON.

In general.—

1. All wrought iron must be tough, fibrous and uniform in character, neutral in quality, and completely welded in rolling, and must have a workmanlike finish. It must be free from injurious seams, especially such as run across the fibre, and must have no blisters, buckles, cinder spots, rough or imperfect edges, or deep creases from tool or roll-guide marks.

2. The finished iron must be straight and out of wind, and must not have over two and one half per cent. of variation between the actual and the estimated weights.

3. All wrought iron when nicked and broken or when pulled apart in testing machines must show a clean fibrous structure, except in the case of the webs of shape iron which may be allowed to exhibit not over 30 per cent. crystalline in the fracture.

4. The finished iron must show no surface evidence of butt welds, and, in the case of wide bars, must have the top and bottom covers in the piles from which the iron is rolled, and, in the case of bars of over an inch thickness, one other bar in the center of the pile to extend over the entire width and length of the pile. Wherever bars of less width than the entire pile are used, the joints shall be well broken in the piling. Material cut too close to the crop ends will not be accepted.

5. All wrought iron shall be made from muck bar or double rolled iron piling pieces, which shall each be the full length of the pile (except in the case of piles to be rolled into wide plates in an ordinary plate mill, where a few alternate layers of muck bar may, and preferably, had best be laid crossways in the pile), the bars in such cases extending completely across the pile, and shall be rolled directly from a bloom made by the puddling process, unless especially agreed upon to the contrary in each particular item.

6. Wherever all single rolled iron or iron made from piling scrap or from re-rolling old rails is to be used, such items in which it will be permissible shall be individually specified in the contract and in the directions to the inspector.

7. There shall be a reduction in the area of cross section between the pile and the finished shape, such that the shape shall be no more than ten per cent. for bar iron, ten per cent. for angle iron, twenty per cent. for beam iron, and fifteen per cent. for channel iron of the original cross section of the pile, and plate iron shall have a reduction in thickness between pile and finished plate, such that the plate shall have not over fifteen per cent. of the original thickness of the pile.

8. Material to be tested shall be of full sized section wherever practicable, and shall be in bars or strips of at least 0.50 square inch in sectional area, and where possible should be at least eighteen inches long, and should be prick punched lightly in such specimens from end to end into one inch divisions on the surface, or, preferably, on the edge of flat specimens, for the purpose of determining the elongation and locating the place of rupture or other defect in specimen. For such purpose the number of inches shall commence at the top or forward end as the specimen is placed in the testing machine.

9. A duplicate specimen of each bar or strip to be tested in the testing machine, except in the case of full sized bars or members, shall be bent cold until it commences to rupture, or through an angle of 180° around a cylinder having a diameter specified in each kind of iron or until the sides are in contact. The bending must stop at the first fracture. The bending may be done in a press or by means of levers or other machines or by blows of a hammer; however, in the latter case the hammer blows must strike upon the outer ends of the specimens, and not at all upon the material undergoing flexure.

10. Test specimens shall in no case be annealed, heated, hammered, forged, or otherwise treated, except where specified in the contract; but shall fairly represent the quality of the material to be tested.

IND OF MATERIAL	Tensile Tests.				Cold Bending Tests.	
	Limit of elasticity per sq. inch.	Ultimate strength per sq. inch.	Elongation in 8 inches.	Reduction per cent. at fracture.	Angle of bend.	Diameter of Cylinder around which the specimen is bent.
BAR IRON.						
Round bars up to 1½ in. diameter..	26,000	50,000	18	25	180°	Upon itself until sides come in contact.
Round bars up to 2⅝ in. diameter..	25,000	Formula.	Formula.	Formula.	160°	One diameter of bar.
Square bars of less than 4 sec. area.	26,000	50,000	16	25	180°	Diameter equal to the side of bar.
Flat bars of less than 4 sec. area...	26,000	50,000	16	25	180°	Diameter equal to 2 times the thickness.
Bars of more than 4 sec. area......	25,000	Formula.	Formula.	Formula.	140°	Two diameters of bar.
CHANNELS AND BEAMS.						
Test from web	25,000	47,000	14	20	140°	Diameter equal to thickness of specimen
Test from flange....................	26,000	50,000	16	25	160°	Diameter equal to thickness of specimen
Angle iron	25,000	49,000	15	22	140°	Diameter equal to thickness of specimen.
Other shape iron....................	25,000	48,000	14	18	120°	Diameter equal to thickness of specimen.
PLATE IRON.						
Test to be taken with the fibre.						
Under 18 inches wide...............	25,000	49,000	14	20	160°	Diameter twice thickness.
18 to 36 inches wide	24,000	48,000	12	16	100°	Diameter twice thickness.
36 to 54 inches wide	23,000	47,000	11	12	90°	Diameter twice thickness.
Over 54 inches wide.................	23,000	45,000	9	12	90°	Diameter twice thickness.

11. Wherever full sized bars or members are tested the material shall be allowed a reduction of 1,000 pounds in ultimate strength, and of two per cent. in reduction of area for each additional inch of sectional area over four square inches. Such bars shall have an elongation of twelve per cent. in their whole length where the sectional area is under four square inches, and of nine per cent. where the sectional area is over four square inches.

12. The testing machine used by the contractor shall be compared with the Olsen Testing Machine of the Pittsburgh Testing Laboratory, and if the results vary the contractor's machine shall be corrected until the tests show that the machines accord. If this is impossible, then the difference shall be equated and added to or subtracted from the results obtained from the machine used by the contractor.

13. Complete facilities for inspection of material and workmanship must be given by the contractor. Facilities and specimens for testing and the necessary labor shall be furnished by him without charge when called for by the engineer or inspector.

When any full sized manufactured iron or steel members are tested to destruction and proved to be up to the standard required, such material shall be paid for at the same rate per pound as the contract price. Should such members fail to reach the standard, they will not be paid for, and the inspector may reject all similar members made of the same material.

14. The acceptance of any materials or manufactured members by the inspector shall not prevent their subsequent rejection, if found defective after delivery, and any such materials or members shall be replaced by and at the expense of the contractor.

TENSION IRON.

1. All material subject to both or alternate compression, and tension stress shall be considered as tension iron in the requirements of these specifications.

2. All iron used in tension must come up to the requirements expressed in the table given above.

COMPRESSION IRON.

1. All iron used in compression must have an elastic limit of at least 25,000 pounds, and a tensile strength of at least 48,000 pounds, except in the case of webs of beams and channels and wide plates, which shall conform to the requirements of such webs and wide plates for tension iron.

2. All iron used in compression may be allowed a reduction of twenty-five per cent. in the elongation and reduction of area of similar material for tension.

Cold bending specimens must bend through an angle of at least 90° without sign of rupture.

RIVET IRON.

Rivet iron shall be subject to the same requirements as tension iron of the same section, and shall further be capable without cracking or serious abrasion of being heated to a good forging heat and made up either by machine or hand work into rivets, and of again being heated to a good red heat and forged as in riveting and allowed to cool, and upon being nicked and cut out of the work it was in, it must show a good tough, fibrous structure without any crystalline appearance. Rivet iron shall especially be re-

quired to be neutral in character and tough in fibre *after being riveted*, and to flow well in riveting.

CAST IRON.

All cast iron used in compression must be of best quality—tough gray iron—and must be able, in bars of one inch square and five feet length, and when supported by knife edges four feet six inches apart, to carry a load of 500 pounds without rupture, and without a deflection of more than two inches.

STEEL.

In general.—

1. The methods of manufacture of the steel whether by the Bessemer, Open Hearth, or Crucible Processes, shall be distinctly specified in the contract for each item of material.

2. Each ingot designed to be used in the contract shall be plainly marked with the blow or charge number in the case of Bessemer or Open Hearth steel, and a portion or the whole of one full sized ingot of each blow or heat shall be rolled into the shape or bar or plate material to be tested, and if it proves unsatisfactory in its tests, the whole blow or heat is to be rejected.

3. If drillings of the steel which were taken from any portion of the finished material do not satisfactorily answer the chemical requirements, the steel of the whole charge or blow is to be rejected, unless it shall be proven that the defect was caused by the particular ingot or bloom having been "burned" in heating.

4. No steel shall be struck with a hammer or worked while at a black heat.

5. The requirements and general directions for size and preparation of test pieces of steel shall be the same as that of iron described before.

TENSION STEEL.

The contractor is to fill in the analysis percentage in the ensuing text both for tension and compression steel in the bid, and shall have made, any reasonable number of analyses of the material that may be required.

Steel for eye-bars and other tension members, and for rivets shall contain not less than $0.\frac{\ }{100}$ per cent. carbon nor more than $0.\frac{\ }{100}$ per cent. carbon, nor more than $0.\frac{\ }{100}$ per cent. phosphorous, nor more than $0.\frac{\ }{100}$ per cent. sulphur, nor more than $0.\frac{\ }{100}$ per cent. manganese, nor more than $0.\frac{\ }{100}$ per cent. silicon.

Test specimens shall bend through an angle of 180°, and bend down upon themselves without showing a crack or flaw, and similar bars shall have an ultimate strength of not less than 70,000 pounds per square inch, an elastic limit of not less that 38,000 pounds per square inch. It shall elongate at least eighteen per cent. in eight inches, and show a reduction of area of at least thirty-eight per cent.

Test pieces shall be of at least one half inch sectional area, and cut lengthwise from finished bars or shapes, or either crosswise or lengthwise from plates of steel used for tension.

All steel eye bars must be annealed in a properly constructed furnace.

COMPRESSION STEEL, BOLSTERS, BEARING PLATES, PINS AND ROLLERS.

This grade of steel shall contain not less than $0.\frac{\ }{100}$ per cent. carbon nor more than $0.\frac{\ }{100}$ per cent. carbon, and shall contain less than $0.\frac{\ }{100}$ per cent. phosphorous, $0.\frac{\ }{100}$ per cent. sulphur, and less than $0.\frac{\ }{100}$ per cent. manganese, and less than $0.\frac{\ }{100}$ per cent. silicon. Test bars of not less than one half inch sectional area shall bend 180° around their own diameters without fracture or sign of crack or flaw, and shall have an elastic limit of not less than 45,000 pounds per square inch, and an ultimate strength of not less than 80,000 pounds per square inch. It shall elongate fifteen per cent. in eight inches, and have a reduction of area of at least thirty per cent.

RIVET STEEL.

All rivets in bridge structures shall preferable be of good quality soft pure steel. All rivet steel shall be subject to the same requirements as tension steel of the same section, and shall further be capable without cracking or serious abrasion of being heated to a good forging heat and made up either by machine or hand work into rivets, and of again being heated to a good red heat and forged or pressed as in riveting and allowed to cool, and upon being nicked and cut out of the work it is in, must show a good, tough, fibrous structure with no crystalline appearance. Rivet steel shall especially be required to be neutral in character, and pure in composition, tough and fibrous *after riveting*, and must flow well in riveting.

Rivet steel should not have over $0.\frac{15}{100}$ per cent. carbon, and must not have an ultimate strength of over 60,000 pounds per square inch, and must have an elongation of at least twenty-five per cent. in eight inches.

DISCUSSION.

MR. ROBERTS—I would like to ask Mr. Hunt in the bending test for angle iron, how he prepares his examples.

MR. HUNT—The test specimens are cut or planed out, usually, from the angle, or whatever shape iron they are cut out of, the thickness of the angle and about 1½" to 2" in width.

MR. METCALF—I have read the record of a great many tests made on both iron and steel at what is called a black heat; a record of a great many tests made in both England and France some years ago and these show, without excep-

tion that iron worked at black heat failed, but every now and then, steel worked at black heat worked very well.

I would like to ask the gentleman why it is that he specifies this for iron and will not allow it on steel, which will stand it better than iron.

Another question; he makes his specifications for bends in iron, a compact fibrous material, which ought to bend well, and generally does, and says nothing about bending it back again, but when he gets to the specifications for steel, which is comparatively crystalline, and very few specimens will bend back again, he requires it shall bend back again, which is a more severe test on steel than on iron. I do not understand the object of these distinctions. Then he makes a chemical specification of which I shall not speak now, excepting that the element of silicon is disregarded altogether, and it is a very important one.

Mr. Hunt—I have had a good many conundrums put at me all at once. As to Colonel Roberts I have answered that in bending tests we have to cut out specimens $1\frac{1}{2}''$ and $2''$ in width which are bent around until they come double. They are all hammered around in a cylinder which shall be at least in the case of angles three times the thickness of the angle.

Mr. Metcalf mentions the requirement that the steel shall be bent back upon itself. I mean by that that the specimen shall be bent over double so that the two surfaces meet, which we do not require in iron. We do require it for steel, as with its crystalline character it is a severer test for ductility than that required for the iron. We use the steel, which costs more money, because it has a greater ductility and we demand greater tests. We do not require it shall be bent down and then back again.

Mr. Metcalf—Perhaps I did not hear straight.

Mr. Hunt—Perhaps I read it wrong. The percentage of phosphorous we mention in our requirements for the reason that not only the quality of metal, but the money value of the steel is largely determined by it in specifications to be bid upon by outside parties, they should be placed all upon the same general basis as in fairness should be demanded by the party who is placing his work. The requirement which we have placed for sulphur, for carbon and for manganese, in our experience is that which has been found to be the limit to get the mechanical requirements asked for.

I agree with Mr. Metcalf that if the material does answer the mechanical test that is about all that we ought to ask. One point, however, may be mentioned, that a sample of Bessemer or open hearth might come up to the requirements and yet 99 samples out of 100 of steel with that analysis would not do it. For instance, you might get a Bessemer metal which would have 2·10 per cent. phosphorous which would answer all requirements of good tension steel, but I do not think you will find the average run of .20 per cent. of phosphorous metal to answer those requirements, and have made a phosphorous limit first, because it was more fair to all parties, and, second, it is almost a necessity to have the phosphorous down to .10 in order to fill the requirements.

Mr. Metcalf—The gentleman does not answer my question at all in regard to the chemistry. I merely said I had nothing to say on the subject. I never presumed to make a chemical specification myself but I admit the importance of it. The question was that he ignored silicon altogether although it is one of the most important elements in steel. I do not know why it should be left out.

In regard to this matter of specifications, there is another set of specifications for steel for structural purposes about being published, if not already published, in which the chemical specification is left blank, and I would say that that was done at my suggestion; carbon to be within certain limits, phosphorous not to exceed so much, sulphur, silicon and manganese not to exceed so much, leaving the matter open to the steel maker himself to say what character of steel he proposes to furnish. Once he puts his figure down on paper he must stick to it, if the purchaser chooses to hold him to it.

Now, as to why I was led to that suggestion, there is a little story connected with it. I required at one time quite a lot of boiler steel in our works. I partly made a friend of mine in Cleveland promise I would order the next lot from him merely as a matter of friendship. The matter leaked out and I received word from my partner that our neighbors and competitors in Pittsburgh were complaining of it. They said if they were not allowed to bid upon this boiler steel and it was purchased in Cleveland, the story would go out and go over the country that the Crescent Steel Works would not buy steel in Pittsburgh for their boilers. They should have a chance to bid. That seemed fair and my partner, Mr. Miller, then asked me to make a chemical specification of the steel, also physical, and ask the different parties to bid on it. That I promised to do and then thought over the matter very seriously as to what these chemical specifications ought to be, and I concluded these people knew better than I and I wrote each of the steel houses in Pittsburgh to give us prices on a certain quantity of steel for quite a number of boilers, making their own chemical specifications.

The result of that was that each party in the city here bid and bid at a very low price. They all underbid Cleveland, and every one sent in a chemical specification I would not like to meet myself, it was so good and so low, all below what Mr. Hunt has specified in his paper. Each one guaranteed that the steel should be within the limits given. That was entirely satisfactory. The bids were mostly based on open hearth steel but one or two on crucible steel.

The party who furnished an equally good specification with any of the others, also the lowest in price, and was entitled to the order, also offered crucible steel at one-half cent per pound higher and the boilermaker urged me to take the crucible, because it was worth more, never had been known to fail anywhere. Well with my well-known love for crucible steel I ordered it. The result was that when I got the tests I found that I had paid more for the steel that was not as good as open hearth. I tell this story for the benefit of engineers outside of the steel manufacturing business who do not generally understand the matter. It is my opinion that engineers will get offers for better material if they will ask the steel makers to make their own specifications, because they will be so anxious to get the trade, especially if a large lot, they will put the specifications as low as they can safely go and once there you have got them.

These specifications are reasonable except Mr. Hunt has left out silicon. They can all bid within those limits; each one knows how cheaply he can work and he wants to get the best for it, whereas the other way the man who has special advantages for making finer material will offer it and the result is the engineer gets the best thing that can be had for the money.

Mr. Rodd—Mr. Hunt has certainly prepared a paper that is a move in the right direction. We ought to formulate the qualities of good iron for bridges and structures. There are some things in the paper that I do not exactly agree with.

I think generally, the requirements are rather low for iron, I believe in wrought iron if you get good material. It is giving us good service in our bridges and structures.

In specifications the more simple the requirements are the better, for obvious reasons. Different shapes of iron, within ordinary limits, I have found in practice, give reasonably near similar results. That is, a good angle, a good channel, a good beam, will give results, which, putting all of one kind together, that is the tests of all your angles, and so on will average fairly together and will not vary very much, provided you have good material.

There are certain specifications which have been in vogue now for quite a long while. Generally speaking they are that wrought iron in small specimens should stand 50,000 pounds with limit of elasticity about 28,000 pounds and elongation say 15 per cent. in eight inches. In large sections, testing in full size, there is a reduction from this. Small bars exceed this.

For plate iron I think Mr. Hunt's specifications are too low. Plate iron is used by connecting it up with rivets to other plates or to shapes. The strength of the structure depends upon how you secure it to the other parts to make a girder, for instance, in a bridge. Mr. Hunt puts wide plate iron way down to 43,000 pounds, and yet it is obtainable in very wide sheets much above that on the average. It can be had at from 46,000 to 48,000 right along; and I think we ought to keep the requirements of iron high as possible. We must inspect it and get the best we can. Engineers generally do not want to specify a thing they cannot get, yet nothing should be yielded of what may be obtained of regular make.

It was a practice some years ago to call for iron 60,000 pounds, and people thought that they had it because they got their result sometimes by testing in the old fashioned groove test, where the small specimen was grooved out in such a way that the strength at the place the piece must break was greatly reinforced by adjacent metal. Most of us are familiar with that method of testing. The plate iron should be somewhat higher than specified, both as to limit of elasticity and elongation.

One requirement of the specification is "neutral" iron. That is what we want. The iron used for tension members is invariably put into a blacksmith fire and sometimes very much damaged there. So far as I see, however, there is no test to establish whether the iron is red short, cold short or what it is. We want iron to work in the fire but not a cold short iron.

In regard to steel I have not had much experience. It seems to me that the limits are sufficiently low and I think that they ought to be kept low, because there has been a great deal of trouble in forging steel. Tension members must be forged in the same way as if they were wrought iron; there has been a great deal of trouble in accomplishing this successfully.

I do not think it can be said that steel tension members have been successfully forged as a rule.

I understand, however, that one of our members, who is also a member of the National Society, will shortly read a paper in which he gives some very interesting information in regard to forging steel.

I am in favor of making sure of what we have, believing that wrought iron for ordinary spans

is very good material. Steel is not as well suited for concussive strains as wrought iron. Wrought iron will suffer from vibration, but steel will suffer more and give out sooner.

Mr. Naegeley—I fully agree with Mr. Rodd. A short time ago I made tests of wide plates from 60 to 70 inches in width which averaged 48,000 pounds ultimate and those were not the only tests I made. Angles as well as channel iron. I think that the specification for bar iron should be kept up and I think that we can easily expect at least 52,000. I have received time and again more than that.

As regards steel I think in reference to analyses that they ought not to be required as long as we have the physical tests. That should be left with the contractor. In that respect I agree with Mr. Metcalf.

Mr. Brown—Why would it not be well to leave out the elastic limit and the tensile strain and use the modulus of elasticity altogether?

Mr. Hunt—We do not ordinarily require this because it does not tell us much of the character of the material. These specifications were with reference to the quality of iron for its work. The stretch and reduction of area are direct factors of ductility of the material, which the modulus of elasticity would not be.

As to the matter of width of plates, it has been my experience contrary to that of Mr. Naegeley and Mr. Rodd, although I admit they have had more experience than I have had, that it has been difficult on the average for wide plates to stand the same test as narrow plates. Plates under 36″ wide are usually made on a universal mill and get better work. We get a very much better test from universal mill plates under 36″ wide than from plates wider. I am trying to make these specifications as reasonable as possible and not place them above what can be obtained on the average with the best materials and best methods of work.

I like Mr. Metcalf's idea of requiring the contractor to state his own maximum limit for impurities in the steel and think we shall incorporate it in our specifications.

Mr. Metcalf—I agree with you that the modulus of elasticity is all you want. Some year or two ago I had a certain lot of samples of steel sent to me, some to be annealed, some to be tempered, some to be hardened, some without any manipulation. They were all to be sent to Watertown to be tested on the big Government machine. These bars were all tested and the tests gave the varying results in elastic limit, percentage of elongation, stretch, ultimate strength, that might be expected from different bars treated in these different ways, but the discovery was made, and a very remarkable one, that when they came to figure out the modulus of elasticity that all had the same one.

All you need, as has been stated in this room though not before this society is to have your spring. However, I will say that springs are not made that way but that is the way the modulus of elasticity will work out.

Another question I think the engineers who are engaged in structural work have given a great deal of thought to. It was very common a few years ago among some of the most eminent engineers to ridicule your factor of safety. They called it a "factor of ignorance." I think it is not a factor of ignorance but a factor of safety. If any one will take the trouble to read carefully the report that Mr. Baker has made on the work recently done on the Frith of Forth bridge, you will see what terrible dangers structures are exposed to from alterations of extension and compression, and when you consider the facts and data given there by Mr. Baker you will see that he wanted a large factor of safety. He makes some remarks there in regard to steel. He is using a very much higher steel in that structure than has ever been used in a work of that kind, except in the St. Louis bridge. There Captain Eads used a very high steel, particularly for compression members, and they are both right. There is a principle there that it would be well for us to bear in mind. That is, that where a structure is subjected to alternate strains of extension and compression, it is under the same conditions as when subjected to vibrations, properly so-called.

That is to say, a slow alternate strain may be considered to be the limit of vibration, quick vibrations giving the limit at the other extreme, and undoubtedly subjecting the material to alternate strains of extension and compression delivered rapidly.

Now it is well known that the higher the material the more rigid it is, the better it will resist vibration. In other words that the resistance to vibration is a function of rigidity, really resistance to shock and impact. You will see why it is perfectly safe to use higher material subject to this alternate strain.

You can illustrate that principle better if you take three wires, one of copper, one of soft iron and one of mild steel and then take a regular piano wire, which is tempered steel. Let them all be the same style and tune them and get them to the same tone as near as possible, then subject them to the pads of a piano and which wire will give out first? Unquestionably the soft. Your tempered piano wire will keep that tone indefinitely. That is the principle involved in the resistance to vibration and that same matter

is given out by Mr. Baker in his report, where the same principle is applied.

Mr. Rodd—I believe Mr. Baker had to use high steel, as also did Captain Eads, on account of the large size of the structures, requiring very large members. In regard to the simile showing steel to be best able to resist vibration. it is more rigid undoubtedly, but the vibration is the result of a shock. It vibrates from the shock. Now, if a shock causes the vibration, and this is certainly the case, it seems to follow that the vibration caused by the shock is injurious; in fact, I believe it to be more damaging to steel than to wrought iron.

In regard to the piano wires, the case does not seem to me exactly to agree with the facts in practice. They were to have the same tone, these different metals, but would have to be strained to different amounts. The piano wire would require far less stretching, I believe, though I may be mistaken, to give it the same tone, than would the copper wire.

Mr. Brown—Each tone is strained to a point proportionally near to its limit of elasticity. I mean just inside the limit of elasticity, and then which wire would retain its tone the longest?

Mr. Rodd—Still each of those has a very strongly defined limit of elasticity. The piano wire has a very high one, the copper wire a very low one, so that I hardly think that parallel will hold, to show that copper or any soft material would not be better adapted to resist shock than highly tempered material, provided section be sufficient.

Mr. Metcalf—I did not say to resist shock. I do not care to be misquoted. I said distinctly that resistance of the wire was clearly a function of its ductility, but there are many parts of structures where it is almost impossible to bring a direct shock on them, but they are always subject to vibration, but I do say it is clearly well established that the more rigid material will resist vibration better. That is so well established now as to be put beyond all question. The function of vibration is rigidity as the function of resistance to shock is ductility. It is well worth while to keep that distinction well in mind, and that where you are safe from the direct impact, the higher the material is the better it will resist vibration.

Mr. Hunt—Do you consider the concussion of a railroad train a vibratory or concussive shock?

Mr. Metcalf—Undoubtedly a concussive shock to the part of the structure receiving the direct blow, but it must be vibratory to the rest of the structure.

Mr. Rodd—The trouble is to determine between the vibration and the shock on any structure, which members receive shock and which vibration, as it is done very suddenly. In a bridge the floor beams receives hocks first, but they do not receive a shock any more directly than many parts of the structure. It is transmitted to the floor beams through some other part, say the stringers, or the rails on which the wheels travel. Now, it is transmitted then indirectly to the floor beams, and yet I believe it gives them a shock. I really believe every part of the bridge, if a moderate structure (in span), received a shock, not only on the floor beams but in every part, when a very large load passes over it rapidly. Every part receives a shock.

I did not in any way wish to misquote Mr. Metcalf, but I could hardly disconnect shock and vibration in my mind, from the fact that shock causes vibration, the vibration being directly the result of the shock. Mr. Metcalf did not distinguish, but said it is not a shock, but admits that the vibration is so violent as to be equivalent to a shock.

Mr. Metcalf—All structures must be designed to resist shocks, to which you know them liable to be subjected. You know perfectly well there is no bridge structure in existence, if subjected to continuous vibration for a considerable length of time, it would not go to pieces. Now then, the point I was making, that Mr. Baker has fully recognized and has given the data for, is the matter of alternate strains, and he also refers to vibrations. I merely said that I think the two things are of the same nature. He speaks of the terrible danger to all structures which are subjected to alternate strains, and he finds the best resistance in the higher steel, and I think Mr. Baker is right. In building that bridge out of the high material he has put up something which will resist vibration better than if mild, and particularly in that bridge where he will have high wind strains and much vibration.

Mr. Rodd—Mr. Baker is undoubtedly right, but the case is so very different from ordinary practice. There you have an extraordinary length of span; so much dead weight that the inertia of the bridge itself is enormous, so that he is safe in using high steel. But to bring that down to general practice in bridge building would be very unsafe. We should try to get the inertia of our bridges as large as possible in proportion to the live loads passing over them, especially bridges used by railroads, where very large loads pass over them at high speed.

Mr. Roberts—The last two or three years in traveling, particularly in the west, I have met a number of engineers. There is a general want of information in regard to steel. They know more about it in this society than probably elsewhere in the United States. I think there would be more steel used in bridge structures if

the engineers felt safe in advising it. It seems some of them are afraid of it. The tests for steel, as read by Capt. Hunt, seem to me about fair enough to go abroad as representing the character of the material to be advised for structures. It does not seem to be too high.

Mr. Zimmerman—There are some points in the paper read which I would like to make a few remarks about. In the first place I believe Mr. Hunt makes an upper limit to his mechanical tests for steel. That I have always found to be very objectionable and I can not see any reason why it should be put in. For instance a steel that is 85,000 pounds in tensile strength, instead of 80,000, certainly is a better material to use in a given place, say the top chord of a bridge other requirements being equal. Steel makers object to upper limits and it was found necessary, in the specifications for the Plattsmouth bridge, for the Bismarck bridge and for the Niagara river bridge, all of which used steel in the top chord and the lower chord, except in the Niagara river bridge where the steel is only used in the compression member, they were obliged to throw out the upper limit.

I think the carbon might well be left out of specifications altogether, as well as the maximum limits on the mechanical tests, but strange as it may seem the specifications as usually written by engineers, particularly, as Mr. Roberts says, in the west, are wrong in probably the most simple test there is to make, and that is the bending test. It is seldom that you see a specification that does not require iron tension bars to bend at least 180 degrees, frequently more.

If you have ever tried large iron bars you will find as a rule bars perfect in every other respect, bars which the physical properties show to be good material, will not bend 180 degrees when they are over say $1\frac{1}{2}''$ in diameter; that is to bend around a cylinder $1\frac{1}{2}''$ in diameter. If you figure the extension of the outside fiber of a bar $1\frac{1}{2}''$ in diameter you will find that that fiber is bound to extend, in order to make that curve, 33 per cent. of its length, and it is scarcely reasonable to ask fiber in iron to stand 33 per cent.

I did not attend as carefully as I might have to the specification but those are the points that principally struck me. I think Mr. Metcalf's idea with regard to allowing the steel makers to specify their own chemical analyses is a good one, although I had never seen it done before or ever heard of it. The ultimate limits I think should not be specified at all and the bending tests from what I remember of the specifications are good.

Mr. Metcalf's idea with regard to high steels for compression members in certain cases I think is right. In fact, I know he is right, so far as resisting rapid vibration is concerned the higher the steel the better it will stand the vibratory motion. The principal member of an ordinary truss bridge is subjected to very small direct shock. The shock transmitted to that member is usually transmitted through a number of other members. For instance through the floor beams to the posts, the tension members and then in a transverse way to the upper chords. It seems to me that the direct thrust on that upper chord is very small and it has been the universal practice, I believe in this country, to make that upper member of much higher steel than the other tension members, but I am afraid of using too high steel, principally on account of its abuse in the shops. I think the men in our shops need to be taught how to use the higher steels and they could work it as safely as mild steels but as yet I do not think they are able to do it.

Mr. Roberts—I am very much surprised here this evening to hear advised by those most experienced on the subject the use of high steel, particularly in compression members. I have got myself pretty well educated in this Society that the ultimate strength should not exceed 80,000 pounds. In any case there is a liability as Mr. Zimmerman says of abuse in the shops.

I can not recall the speakers at that time but I think there has been a change in opinion, but I think possibly Mr. Metcalf has changed when he now advises using high steel.

Mr. Naegeley—There has been so much improvement of late in the manufacture of steel that we can put more reliance in that material than heretofore. I should advise more confidence, particularly among engineers who have it in practical use.

Mr. Rodd—With regard to high steel—it is mostly to repeat what I have said. High steel means smaller sections. It answers for very large structures but not for small ones. You want steel of pretty good thickness. That is needed on account of connections and to last well. The chords spoken of are generally made rather large, so as to take advantage of a certain factor of compression members, which is alluded to very frequently by engineers, called "the radius of gyration". That "radius of gyration" is a factor of the allowed strength of a compression member. The effect of introducing it is to thin out the metal very much. You want to get as much metal away from this radius of gyration as you can. It counts more. All that goes to reduce the thickness of the material you use which, as I say, may be good in a very large structure but not in a small one, or one of ordinary size. Now, in regard to the specifications—you have to build your structures—you want good materia

for them; it has been the case in the last ten or more years, at least two or three times, that all the mills in the country were very busy and although they wanted to serve their good customers, they were unable to roll the iron for them promptly; if iron was rejected then serious delays occurred; it is essential to be able to get the iron you specify. It follows that you should specify material regularly obtainable yet keep up the standard fully.

One way to accomplish this, in fact the way I have done myself, has been to take the results of tests for a series of years, say as much as six or seven years, these tests including in all cases the ultimate strength, elongation in 8″ and limit of elasticity, for each kind of iron; having now perhaps 500 tests of each kind of iron, throw out all very high results, then the averages of the remaining tests will assure you as to the iron you have obtained. It is my opinion that the standard should be kept up and not lowered, so that you should specify at least as good iron as you have been getting regularly under rigid specifications.

Mr. Dagron—I have not read the paper of Mr. Hunt very thoroughly but I see in the first part of it Mr. Hunt specified the manner in which the manufacturers shall prepare their piles, the amount of reduction they shall give from the cross-section of the finished piece, and also as to what items shall be permissible, made of old rails and from scrap.

I think Mr. Hunt exceeds the powers of an inspector in this regard. I hold it to be the province of an engineer to make as rigid tests as he chooses, but that he leave the mode of manufacture to the manufacturer. All in his province is to see that he gets the material that corresponds to his specifications. In doing that he fulfills his duty. In going beyond that he exceeds his duty.

Another point I would call attention to is section 12: "The testing machine shall be compared with the Olsen Testing Machine of the Pittsburgh Testing Laboratory, etc.,—until the tests show the machines accord."

This is making the machine of the Pittsburgh Testing Laboratory the standard for all other machines. We have no evidence to show that this machine is more correct, and it would be well to determine that before sending this specification forth in this remarkable manner.

Then I notice also in the case of bars 1½″ in diameter, they shall bend 180 degrees around their own diameter, until the sides come in contact. I do not think there is any iron made that will stand that treatment—"until the sides come in contact". At least it has not been my experience to find that quality and I have had iron from the best mills of the country.

He also reduces the specification for elastic limit as the thickness of bars or the diameter increases. I think this is a mistake, because the thicker the bar proportionately there is less work upon it and therefore its texture approaches closely to that of cast iron, in which the elastic limit is almost up to the ultimate strength. The elastic limit increases instead of decreases, therefore I do not see any necessity for reducing it here in the specification.

In wide plates it is the same thing. The elastic limit goes down to 23,000 pounds. It is as easy to obtain 25,000 to 26,000 pounds in wide plates as in smaller plates, say 12″ in width.

Now as regards steel. The third section says: "If drillings of the steel which were taken from any portion of the finished material do not satisfactorily answer the chemical requirements, the steel of the whole charge or blow is to be rejected, unless it shall be proven that the defect was caused by the particular ingot or bloom having been 'burned' in heating."

I do not believe in specifying the chemical requirements for steel. If you get the physical requirements that should be satisfactory. It is easy enough to make an error in the chemical tests, and while the error may not be a great one it would be a hardship to the manufacturer to throw out a lot of steel because it was lacking a little in carbon or phosphorus.

And as for rivet steel he says: "the steel shall be pure steel." I would like to know what pure steel is. It says: "steel to be neutral in character and pure in composition." I would like to know what pure steel is.

Mr. Hunt—Mr. President, I have just said to Mr. Metcalf that I think he is the authority on steel in this Society and I would respectfully ask him to give a definition in answer as to what "pure steel" is. What we meant by it here is steel pure from impurities, sulphur, phosphorus, or such impurities as would render it cold-short or red-short.

Mr. Zimmerman—In Mr. Dagron's discussion just now I heard him make the remark that the elastic limit in cast iron was very near the breaking load. My experience with cast iron, although not possibly as great as others here present, has always gone to show that the elastic limit of cast iron, instead of being nearer the ultimate load of the iron, is very near the initial load. The fact of the matter is it depends a good deal on the definition of "elastic limit", as to whether you consider it as the point beyond which the extensions cease to be proportionate to the load.

It is very certain that cast iron, for every additional load, has a different extension. The cast iron curve has not in it any part perfectly

straight, as in the curve of a test of wrought iron or of a test of steel. Every additional load that you put on in tension on a piece of cast iron makes it extend greater than an equal load previously put on.

The consequence is the elastic limit of cast iron can not be determined at all. I have never seen it yet in samples 8″, 10″ or 12″ long, where measuring extensions for every given load, say 500 pounds, the elastic limit would not be in the first 500 pounds.

Mr. Dagron—You can find information on that subject in the Government reports, on testing metals.

Mr. Zimmerman—Not in the last report.

Mr. Metcalf—I think that Mr. Zimmerman is pretty nearly correct in his statement, so far as my experience goes. In the early days of the war I tested myself some thousands of specimens of cast iron, of the very best we could get in the country. At one time for a period of three months I spent the whole time at the Watertown Arsenal, testing specimens 8″ and 10″ long, of various kinds of mixtures of iron with a view to enlarging the number of kinds of iron we could use for guns. I have a book containing all those tests, giving every detail complete, with the loads measured at every thousand pounds, and also the curves plotted, also the comparative curves of all the different specimens. I am sorry to say that I do not know exactly where the book is now. I would like it to be in the Library of this Society, because it covers a good many tests, carefully made.

Speaking after a period of nearly 25 years, and of course from recollection, my impression is we were always able to measure the permanent set under a load of two or three thousand pounds. The cast iron in nearly all the specimens we tested up near the breaking load, where the increment of permanent set changed very suddenly.

I think that Mr. Zimmerman is right in regard to the increased increment for every increase of load, or very nearly so, but there was a point in nearly every one of those specimens, perhaps three-fourths of them, where the increment increased and changed very suddenly before the piece broke down. There was a very decided change at that point both in extension and compression members.

That might be considered the point where the destruction of the material really began. Then the elastic limit would be up close to the breaking strain, and if you consider the elastic limit as the point where the permanent set is measured, it is generally very close to the initial load, as Mr. Zimmerman stated.

I may say, in answer to Mr. Hunt, as to what is "pure steel", that I can hardly say I know. I have a theory that pure steel should be iron and carbon in certain proportions. I regard any steel as practically pure which contains less than .02 per cent. of any impurity, and that is a point almost impossible to reach. It can be reached, though. I do not think that such extreme purity is necessary, even in rivets, because if the carbon is very low and the steel does not contain oxide of iron, which is the most fatal enemy that steel has, then a reasonable amount of phosphorus and silicon can be endured in the steel and it will have very great ductility and great powers of endurance.

But in speaking of the chemistry of steel at all I would remind engineers that really the most dangerous enemy of steel is oxygen. As our lamented friend, Mr. Park, said at the first meeting of this Society, it is the great enemy of steel.

In regard to making any chemical specifications of steel at all, I understand Mr. Hunt has changed his specifications slightly from what they were originally and leaves that matter to the manufacturer.

I would remind Mr. Dagron of one thing, that while he is correct in his position that if the material will give the physical tests required it ought to satisfy the engineer, and so it ought as a general rule, yet steel is subject to such great variations of quality and strain in the hands of manipulators, in the way of annealing, etc., that it is very questionable to my mind whether some of these elements of impurity, as sulphur, are not elements of disintegration. That after steel has been subjected to use for a long time and to the changes of temperature incident to our climate, varying from 100° to way below zero, whether in the end the effect will not be shown by causing a breaking up or disintegration of the material. You know very well we can produce all sorts of changes in steel by simply subjecting it to varied temperatures, and we can make very poor steel appear pretty good by skilfull handling and manipulation.

It is a question whether these elements ought not to be kept down as low as they can be consistent with chemical reasonableness from the fact that there may be some elements of disintegration there with which we are not familiar. This is simply a hint—not even a theory. The steel is subjected to such enormous changes in quality, due to very moderate changes in temperature, that these are questions well enough for engineers to think of and discuss, and some day we may have further light as we learn more about the material.

Mr. Hunt—Mr. Metcalf is right in his statement. We have accepted the suggestion made by him at the last meeting and have placed in

our specification a blank for the contractor to put in his own specification of what he would furnish in supplying the material—what analyses he would guarantee—believing that to be better than to require any minimum or maximum point of composition, and as the contractors will want to make as favorable a showing as possible in their bids, they will naturally place their chemical requirements as low as possible and will place them in many instances far lower than we would in our specification.

As to the other matter of old rails, in wrought iron material we have placed that in as a matter of fairness to the mills themselves. Two different mills make a bid for the same work, one using in composition old rails, the other using the best of muck bar double rolled, we think the fact of the one using a cheaper grade of material should be put in the bid. Therefore we placed it in that way, and while we do not say they should not use old rails we simply say that when they are used that in fairness to other contractors who use a higher cost product, even if not any better, then the fact should be so stated in the bid, and further we shall make twice as many tests and a much more rigid examination of the material if we know that inferior stock is used or not a sufficient work given to the metal.

For while we do not dispute that *it is possible*, due to patent nostrums or "Skillful manipulations," to make good iron out of poor material not sufficiently worked we must say that we are very skeptical in such cases and will need like doubting Thomas "to see and to feel" before we can believe in them. The tests that are usually made by inspectors are not sufficient to always develope treachery, that in our experience will sooner or later develope in poor quality metal designed to be used in places requiring the best grade, and we hold it to be an all-important point that the stock used and the method of manufacture should be in every way above reproach in structural material to which often is entrusted many human lives. I am reminded in this of the saying of an old iron master that "if you put the devil into iron his Satanic majesty is sure to stay there and to show himself when he gets good and ready."

Mr. Franks—I would differ very materially with the gentleman who has just spoken on that point. I think this is due to the skillful manipulation of iron and steel. If one manufacturer possesses that skill which another does not, and it enables him to use a cheaper material to start out with, and yet produces the same result, I do not believe it is fair for him to expose his business in the specifications. If the engineer gets material that meets his requirements it should be satisfactory. The question raised by Mr. Metcalf, if there is any disintegrating material in the old rails that it should be taken into consideration is a proper one. I do not know whether the gentleman who has just spoken did take that into consideration but it has been shown that they can produce excellent material by using old rails in connection with other selected stock, and it should be permissible. They can not work miracles but they can work other stock with old rails and produce as good stock as other manufacturers with double rolled bar. Therefore I think it would be unjust to require any explanation as to how the manufacturer produces the material provided he reduces the material required.

Mr. Zimmerman—There is only one point in regard to this, and possibly other cases, that the manufacturer may be able to produce certain results on a testing machine, and would not be able to produce the same results in the structure. For instance, you take iron for an eye-bar. Eye-bar iron may be made from old rails, or from double refined iron or may be made from red-short, cold-short or neutral iron. They may all by manipulation, or a certain amount of rolling in each case, give approximately the same results in a testing machine, but when you come to work that into an eye-bar subjecting to heating and hammering, the resulting bar would be far different from the test of the small bar. We ought not to expect more than 10 per cent. difference between the sample bar and the bar tested in the machine.

The same refers of course to rivet iron and rivet steel. Nearly all rivet iron will stand well in a testing machine, showing very good elongation, very good ultimate load, but if not tested by some fair test the inspector accepting that material has no idea how it will work after it goes into the bridge.

Mr. Dagron—As regards that eye bar, you allow 10 per cent. on 50,000 pound iron and you get it down to 45,000 pounds. It is easy enough to check up whether the work has spoiled it, if that iron will not stand heating and welding. I have had cases of that kind where the iron stood a test very satisfactorily, giving 52,000 pounds ultimate, but when pulled in full size specimen it broke very low, and then I took the original bar, cut out pieces 12″ long the entire width of the bar, split them in two, had them welded together and pulled in testing machine, and they broke clear across the weld, giving an ultimate strength of 39,000 pounds, showing in that case the iron was red short. By means of full size tests you can check up the manner in which the iron will show up in the structure.

Engineers' Society of Western Pennsylvania.

Report of Sixth Annual Meeting, held January 19th, 1886.

The meeting was called to order at 8.20 P. M., the President, Geo. H. Brown, in the chair; fifty-six members present.

The minutes of the last annual meeting were read and approved.

The President read his address, which was received with applause.

Mr. Lucien Scaife, from the committee appointed to consider the invitation to join the movement for effecting a change in the present method of carrying on public works, reported in favor of the Society taking action.

On motion of Mr. Metcalf, the report was received, and made the special business of our February meeting.

The reports of the Treasurer and Secretary were read and filed.

The election for officers for the ensuing year being in order and no candidates other than those presented at the December meeting offering, on motion made, seconded and adopted by a standing vote, the Secretary was instructed to cast a ballot electing them: which being done, the President announced the following gentlemen as the officers for 1886:

President, E. B. Taylor; Vice President, S. B. Fisher; Directors, W. L. Scaife and F. C. Phillips; Secretary, S. M. Wickersham; Treasurer, A. E. Frost.

On motion of Mr. Metcalf, it was resolved, That the time for holding our next meeting be changed from the third to the second Tuesday of February, in consequence of the "American Institute of Engineers" meeting in Pittsburgh on that day, and a cordial invitation be extended to them to make use of our rooms as they see fit; and a committee composed of Messrs. Shoenberger, Rodd and Taylor were appointed by the President "to welcome and extend the hospitalities of our Society to the American Society of Mining Engineers."

The Board reported favorably on the following applications for membership, and they were balloted for and approved, viz.:

C. M. Bennett, W. C. Stevenson, Alonzo H. Brockett, A. W. Cadman, Richard Nevins, Charles C. Bothfield, W. A. Geisenheimer, Howard M. Wilson, James Hetzel.

A recess of ten minutes was taken, when on again coming to order, Mr. T. P. Roberts read a paper on "Long Distance Transportation of Natural Gas."

Mr. Jarboe opened the discussion on the subject, but on account of the lateness of the hour the meeting adjourned.

SAML M WICKERSHAM,
Secretary.

Engineers' Society of Western Pennsylvania.

LIBRARY AND ROOMS,
GERMANIA BUILDING.

SECRETARY'S OFFICE,
411 WOOD STREET

SYNOPSIS OF DISCUSSION ON REPORT OF COMMITTEE ON STATUS OF CIVIL ENGINEERS IN GOVERNMENT SERVICE.

PITTSBURGH, February 9th, 1886.

Mr. W. LUCIAN SCAIFE: When this subject was introduced, it seemed to be the idea that its object was to get the civil engineers to take the place of the military engineers, to raise their wages and better their position; but now a broader ground is taken, laying aside all personal interests, to see if the present is the best system at the country's disposal for carrying on public works. Whilst West Point has produced many bright engineers for military purposes, it may well be thought, that if the time devoted to those studies had been given to civil engineering, they would be better prepared to fill their present positions than they now are. The military needs of the country are insignificant compared with its civil wants.

Mr. FISHER: The other societies of engineers have taken a broad view. The improvement of the whole Mississippi valley looms up; this whole work is in its infancy. Nothing has been done, compared to what will be; the work will go on until the whole net work of streams will be developed.

Mr. ROBERTS: I was not at first in favor of this movement, but I have changed my mind after reading the circulars and proceedings of the Cleveland Engineers on the subject. Let us consider one fact: The Government appropriates annually ten to twenty millions of dollars for the improvement of rivers and harbors, and the number of United States Engineers who have charge of the expenditures of that money amounts to only seventy. They must, therefore, have first-class assistants (civil engineers), but as things are now, whilst it is necessary to employ civil engineers, and often to give them almost unquestioned authority over the sub-districts, yet under the regulations they are only subordinates, with a subordinate salary; for this reason, the Government not paying for it, does not get the best civil service. I think that there is no objection to these seventy men controlling all the movements, and being the leaders in all the works. I favor the appointing a committee to join with other committees of other societies, and report observations from time to time.

Mr. Dempster: Five or six years ago, the conflict between Mr. Eads and the Government, about the jetties, initiated this movement, but after two years' correspondence, from courtesy to the military members of the American Society, it was dropped. The Interior, Treasury and War Departments have now professional men in them, from civil life, who direct the matter of public buildings and architecture. In the improvements at Galveston harbor, thirteen million dollars were expended, and the work so poorly done that it has been entirely swept away. Is it not our duty as professional men, to lend our influence to the end that the public works shall be put on a common sense basis?

Mr. Scaife: The time for action has come! Let the committee have power to proceed to carry out the recommendations of the Cleveland Convention.

Mr. Roberts: Whilst the committee has power to co-operate, which word means a great deal, I think the Society should have always a check line on the committee it appoints.

Mr. Buel: It is expected by this movement to bring about a national system of public works.

Mr. Davison: I think all societies should take a hand in this work.

The debate was further continued by Messrs. Buel, Scaife, Brashear, Jarboe and Dempster, when the following resolution was adopted:

Resolved, That the committee correspond with and keep posted on all going on in the matter, and report to the Society what passes on the subject.

S. M. WICKERSHAM.
Secretary E. S. of W. Pa

Engineers' Society of Western Pennsylvania.

PITTSBURGH. PA.

LIBRARY AND ROOMS.
GERMANIA BUILDING.

SECRETARY'S OFFICE
411 WOOD STREET.

SYNOPSIS OF DISCUSSION AT THE MEETING HELD MARCH 16, 1886.

When "Unfinished Business" was called, the President proposed the discussion of Mr. Roberts' paper on "Long Distance Transportation of Gas."

Mr. METCALF regretted that Mr. Brown, who says Mr. Roberts is all wrong, is not here to-night.

Mr. DANSE: If we may liken gas to a small boy, it is easier to pull than to push him. From Mr. Roberts' illustration of the facility of ventilating mines by suction, and that, when the mines are very tortuous, and the walls much rougher than the conduits to be used for gas, and frequently very long, would seem to point to suction as the preferable method. A good ventilation is got with a very small fan, and to use a low pressure has many advantages. Indeed, I would like to hear any argument in favor of high pressure, which up to this time I have not heard.

"THE NEEDS OF OUR SOCIETY."

Mr. PHILLIPS, Chairman of the Library Committee: Except for journals, no money has been spent on the Library for some time, and yet the additions to it are not discreditable. Probably few realize the value of our collection, but we ought to bring it up nearer to the times. There are many late works we should have, but we have not the money to buy them. The Treasurer informs me there is $1,600 due the Society; if that was paid in, we could purchase some valuable works.

Mr. METCALF: I have thought a good deal on this subject, and have one or two pretty clear ideas: First to be done, is to get that money, and the Secretary might hire someone to collect it; the next thing is to get more papers; but the real need of the Society is more social contact. With good papers and good discussions we feel that we need but little more. Some may think it is creditable to the younger members to give precedence to the older ones in the debates, but I do not look upon it in that light. Let the younger members, like those of the Engineers' Society of New York, meet once a week socially, when often more good is done and more learned, than at the regular monthly meeting; and such meetings tend to stimulate their courage to take their place in the formal debates, and thus add much to the general interest of the Society. Our Library is carefully selected and valuable, and for its size has no superior in this country. The young engineers can find much of value there, both of records and of periodicals. Much good will no doubt arise from forming a social club, by having a weekly meeting.

Mr. FISHER heartily favored the project; he was a frequent visitor to the library, with benefit. Young men would find an advantage in going to the library and studying the works there preparatory to the discussions in the Society, say, for instance, as on the sewerage question; and old members would not be hurt by doing the same.

Mr. Scaife: I think the project will benefit the Society. Perhaps like the Amateur Photographers, our President might appoint a member to give an exhibition of drawings made by him, or some researches on some particular subject; or like Mr. Brashear did one night, a very interesting exhibit of his own handiwork in astronomical matters.

Mr. Awl thought it was to our disadvantage, having our place of meeting and our library in different places; better to pay more and have the rooms connected.

A Member: If the rooms were connected we could keep them open every evening. I would willingly take charge one evening in the week, and no doubt others would do the same.

Mr. Metcalf: Some years ago, this same question was agitated, and it went so far that designs for a fire-proof building were made, estimates prepared, and steps taken towards raising the funds neces-sary. I believe that we would have succeeded at that time but for the unfortunate circumstance of Mr. Carnegie's offer. He sent word to the Society that he intended to recognize the Society in the Board that was to have charge of the building, and begged us not to go on with the movement. It therefore seemed wise not to make any further effort. I still hope to see this Society have its own building; but to-day we have not the revenue to pay for a hall and rooms contiguous. If we are to have these social meetings, the younger members particularly must take hold of the matter; appoint some evening for holding them, and it will not matter at all the location of the room; and once started I think it would go on by its own momentum. The New York Engineers hold a social meeting every Saturday, with good effect. We have a room paid for, and if the members once get in the habit of meeting gentlemen there for a few hours' pleasant conversation, I think it will prove a good thing for us all. I do not think it necessary to pass a vote of thanks to the American Iron Association for the use of this room. They are glad to have us use it. They realize the importance of having good engineers. I do not think they look for any vote of thanks. These very same gentlemen will eventually furnish us money to have a hall of our own. In our former designs the thing could have been accomplished for $30,000, including the lot, and when the time comes again, I think we can raise the money without trouble.

Mr. Wickersham: I heartily approve the suggestions of Mr. Metcalf. The success of the Society depends more on the sociality of the members than on any other one point. As regards finances, which so often make or break enterprises, this Society need not fear. We have three hundred and twelve members, contributing annually $1,500, when all pay up. To-day we have $400 or $500 in the treasury, and about $1,600 to come in. The back dues of the past two years only amounts to about $500; as it not possible for all men to be always easy in money matters, this is not a large sum to be in arrears, considering the number of our members, and there is scarcely a debt on the list but we will get. Our outlay, I think, does not exceed $1,100 per annum; our income, on paper, $1,500. So our income exceeds our outgo, even if things remain as now. Our rooms are good for books, but unhandy to reach, climbing so many stairs. The record shows that the visitors average about two per day, Mr. Fisher being the most frequent. If the young men desire at any time to have a social meeting in the rooms, as has been suggested, and will notify me, I will see that they are lighted and kept open for them.

A motion was then unanimously adopted to hold a social meeting at the library rooms on Thursday evening, at 8 P. M.

On motion, adjourned.

S. M. WICKERSHAM,

Secretary.

EAD'S PROPOSED SHIP RAILWAY ACROSS THE ISTHMUS OF TEHUANTEPEC.

At a meeting of the Engineers' Society of Western Pennsylvania, held June 18, 1886, Mr. Alex Dempster offered the following preamble and resolutions:

WHEREAS.—The very eminent civil engineer of America has undertaken to construct a ship railway across the isthmus of Tehuantepec, which, when completed and in successful operation, will be a vast benefit to the commercial nations of the world—more especially on our own and the great Mississippi basin of which our own valley is a part: Therefore,

Resolved, That this society expresses confidence in the engineering skill of J. B. Eads, to overcome all the physical difficulties that may be presented, and in his efficient business qualifications to apply judiciously the necessary funds.

Resolved, That we request our representatives in Congress, to aid the said project by working for the passage of the bill now pending for that purpose."

DISCUSSION.

MR. DEMPSTER:—I may make a remark or two on this topic. There has been, as we are many of us aware, an inclination on the part of some to belittle the ability of Mr. Eads in this matter, growing out of the rivalry that has existed for many years between the military and civil departments of engineering, more especially that between us since the undertaking of the Jetty system.

There is a rivalry in different locations. You know that our eminent countryman, Mr. Thompson, who used to be in the cabinet, who is now the financier, or has been the financial agent of De Lesseps in the Panama Canal, and there has been an attempt on the part of some to cut a canal across the Isthmus of Nicaragua, but with these there have arisen difficulties which seem to be, if not insurmountable, at least hard to overcome, and bills have been before Congress to abet these schemes which have been defeated before, but the bill which is now before Congress, in aid of the Eads Ship Railway guarantees a certain amount (I have forgotten the amount) which is conditional on the construction and successful operation of the road. Not a dollar of United States money is to be put into it until the road is fully constructed and shows that it is a success—then the United States guarantees, I think to the extent of seven millions and a half of dollars only, and that is the bill that is now pending before the Senate of the United States.

The condition, I repeat, is that the road shall be constructed of other money than of United States money, and it shall be in successful operation before there is a dollar given to aid the enterprise, then there is a certain amount given yearly, which is not to exceed 7½ million dollars, for the purpose of aiding the road, and in return American commerce is to be transported at 25 per cent. discount below the regular tolls of the road. and it is to carry the mails free.

This is the bill that is pending and I think that it is our duty to second the effort that was made by the citizens and the Chamber of Commerce in giving encouragement to the enterprise, in asking our representatives to work for the passage of that bill.

If Mr. Eads fails to get the money that is required to construct the road—if he fails to construct the road or if he fails in its successful operation, then not a dollar is to be given by the United States. But if he constructs the road and puts it in successful operation and carries out his obligation to let American commerce be carried across at a discount of 25 per cent., then the United States is to give a certain amount annually for a certain number of years, said amount not to exceed 7½ millions dollars.

I think it is a very reasonable request of the United States and I think we of the city of Pittsburg ought to express our confidence in the ability of Mr. Eads as an engineer, in the project itself and in our inclination to get that prosecuted if possible.

I may say in this connection that so far as the Panama Canal is concerned it is a long way in the distance. There is an American by the name of Bigelow, I do not know his standing in business or whether he is an engineer, but at any rate he was sent down by the New York Chamber of Commerce to look into the matter and report the condition of the work so that they can be enlightened. I suppose their course, in some manner at least would be dictated by the report he would make, or controlled by the report. That report was to the effect that about one-

fifth of the work had been done and already four-fifths of the 120,000,000 appropriated for the work had been spent. Now a good many of us know, and no one better than Mr. Chairman himself, how estimates fail in the prosecution of large work, that is, that they overreach the approximate estimate unless matters that are unforseen are accounted for, which can not be done. This work is just in that condition. De Lesseps is trying to get money. Why even the commissioner sent by the government of France has made an unfavorable report and I do not think it is very much to say, I do not think it requires very much prophecy of foresight that that will be a long time in completion, that the condition is such that financial men are failing to come up with the amount of money required, for if one fifth of the work is done and four-fifths of the money spent there is a very poor prospect of completion.

I believe the estimated cost of the Eads' work is about $100,000,000. If Mr. Eads, or any other party, begins, constructs and equips that road I think he ought to be encouraged to the extent of $7\frac{1}{2}$ millions by the government of the United States. All the inhabitants of the country will be benefitted indirectly, at least, because it will be a reduction of the tariff that is placed on commerce, which we all have to pay, in an indirect way, in paying for the products.

Mr. Davison:—Suppose his road is like the Panama Canal and costs more than the estimated one hundred millions.

Mr. Dempster:—Well, suppose he fails to construct the road, no matter how much it costs to put in successful operation, until he does this no money is to be given him.

Mr. Danse: —I have been very much interested in the remarks of the gentleman on my right here (Mr. Dempster). We have been told pretty frequently that Eads was going to build this road, that he was able to build it, and all that kind of a thing. I do not think any of us doubt Eads' ability, so far as adopting the resolution is concerned. I guess we are all willing to admit his ability as an engineer, but I think that most of us are pretty confident that while the thing is undoubtedly feasible and will probably be a success, so far as merely operating is concerned, we are all pretty sure that it will never be a financial success, and it is a question in my mind whether the United States Government should undertake to bolster up a thing of the kind, and on that account I am opposed to voting in favor of the resolution.

Mr. Dempster:—So far as the project is concerned it is not nearly so visionary as was considered the jetties of the Mississippi and the parties that offered opposition to them from the beginning were as confident as the gentleman they would never be completed and so far as his doubt is concerned neither he nor I can appreciate the full force of the benefits that will be derived from it. The Suez Canal has been a success, and this project I think will be proportionate to that in success because the commerce will come in this direction rather than go around the other way. By this plan ships can go across the isthmus without breaking bulk. The transportation from the East Indies, I think, from China, from Japan, will be much more likely to come this way than go the other way to the markets even of Europe, and I think it is a very small matter to do. If Mr. Eads fails there is nothing lost—but his efforts.

Mr. Becker:—I doubt very much the propriety of the society, as such, undertaking to lobby a scheme, for any purpose. Personally, I would not have the slightest hesitation in using any influence if I had any with any member of Congress towards the furtherance of this project, and I think that every other member should have the privilege of doing so, if he sees proper, but we should not lose sight of the fact that there are at least three distinct canal schemes now, either in actual progress of construction, or at least under consideration. There is very eminent talent enlisted in all of them and the opinions of very many prominent engineers are divided very much as to the feasibility, practicability, etc., of these three projects.

I think that it is not likely that any engineering society in the country would unite in the recommendation of either of these schemes, no matter what the individual members might think of it. I do not think the society, as such, should be induced or persuaded to volunteer any expression in favor of either one or the other.

I have within a few days past received some communications from a friend, whose opinion I value very highly, who has been personally engaged in the service of the Nicaragua Canal scheme and he is decidedly in favor of that scheme. I have friends, on the other hand, who have warmly endorsed Mr. Eads' scheme, among others I might mention the President of the American Society, Captain Flad, who is perhaps as competent to judge as anybody I know of. I cite these things merely to show that opinions differ regarding these measures. I do not think that the society, as such, has any busness to express its general opinion or views on the subject. It can only be a divided one at best.

Mr. Danse:—The gentleman who presented the resolution seemed to have a very decided

doubt in regard to the construction of the Panama Canal, and then he afterwards spoke of the opposition to Eads in building the jetties. Perhaps if he will put the two things together he will remember at the time the jetties were built the people doubted the possibility of their being a success, just as much as anybody may doubt the possibility of the Panama Canal ever being a success, or of success anywhere in the near future, and it seems to me it is a poor rule that does not work both ways for the arguments brought against one scheme might apply equally well to the other.

On the other hand, as against the arguments we have the Panama scheme started. We do know that one of the heaviest outlays of that scheme was for the enormously expensive machinery, such as had never before been used on engineering work of the kind, and that being the case I think we ought to hold on for a while before condemning work of that kind.

As I have said I have not the faintest doubt of the possibility of building Mr. Eads' railway. I do not think any of us have, but I am very confident for my part, that it will not be a financial success. We have not any authority in the matter, only that it was based on the same way as the Panama Canal now is and that Ead's jetties were. Therefore I quite agree with the gentleman who spoke last that the society should not take any action in the matter.

Mr. Jarboe:—So far as the two canals are concerned, Suez and Panama, the one was digging a ditch through a sandy desert, no hills; the other is digging a ditch through a mountain range. Why the other day only the Panama river washed in, in a flood, as much dirt in one day as could be removed in three. In the Suez Canal the greater part had been dug once, but in the Panama Canal there is the Gorgona (?) line, when they come to that part of the canal there will be rocks there and they will have to make it some 600 or 700 feet deep.

I have been over it, have surveyed the line, have been over it on horseback, on the railroad and have walked over it and know just what is before them.

Mr. Dempster:—It appears to me that because we cannot recommend all these schemes at once it is no reason why we should not encourage one, and that the one we are most interested in. And I do say it will be no harm for the Society to pass this resolution and show our disposition to aid one along. Because there are three horses and we only ride one that does not say that we do not want to ride the other two, but we cannot ride two or three at once and that is the reason I offer this resolution because the action had been taken by the citizens before relative to it and I thought that this society should give the encouragement of its vote to the same purpose.

Mr. Wickersham:—As the seconder of this resolution I suppose it becomes me to give some reason why I did so. Whether the thing is a success or not is not the question. Here is a project proposed by an engineer who has won our confidence by the works he has done. He proposes to build this work which, if successfully accomplished, is going to be of great benefit to us all. I think there is no difference of opinion in regard to that. If successfully accomplished it will be a great benefit to this country particularly—probably to all the countries of the world, but particularly to us.

Under this arrangement we incur no peculiar danger of being called on for any money unless it be a success. If it is a success who is there among us would not want to have the whole of it, the entire control of it, own it all, because if successful the tonnage that will pass over that road will certainly pay as handsomely as anything in the known world, even if the building of the other two routes is accomplished and the canals made, for there is enough travel in the world passing east and west along those three lines to make them all profitable, if all are successfully finished, and that they will be, I do not doubt.

Speaking of the Panama Canal, although it may cost—suppose it costs double the estimate, the French government is committed to build it. They must build it, for they are not like the government that started the canal across the isthmus of Corinth which stands to this day, century after century, a monument to a great work unfinished.

Do not believe that the French Government of the present day is going to let anything stand in its way when so much money has already been expended. They have confidence in its success. They will finish it, gentlemen, do not be afraid, they will finish the canal across the isthmus of Panama, and any one who calculates on that being abandoned may as well give that up, because the French Government is bound to build it, They have the ability to do it for any nation that could carry on such a war as they did in 1870–1 and in a few years cast off every vestige of that war could build several Panama Canals and not feel it.

I think, gentlemen, that under the circumstances we ought to give every encouragement to Mr. Eads in this work. We ought to say that we think it is practicable, and why do we doubt its practicability? He has shown us plans that

no engineer that I know of has gainsaid the correctness of, or criticised as being foolish. I have heard nobody object to it as a piece of machinery. Look at it. There is nothing but a piece of machinery. No one has said "There are certain reasons why it will not work." No, nothing of the kind.

There seems to be some jealousy on the part of each project against the other, all of which I think is wrong. Let each go to work and build their own project. I would not like to put money into this project, but that is unnecessary. He asks us to do so if he is successful. He is an American. He is one of us. Let us favor him with our little voice. To be sure it won't go far or carry a great ways, but it will have some weight and I think as far as we can go let us go, and say go ahead to Mr. Eads and say to the government "help him all you can."

One of the first lessons I got in engineering as a lad, was copying the drawings made by Mr. Strickland who was sent out by the State of Pennsylvania to study the canal system of England and to come home and report; that we might try the improvements, if there was anything better there than here, and I have always taken an interest in canals since. Now if the railroad across the isthmus of Tehuantepec can be built, as I believe, for the man that built the St. Louis bridge, I think can build most anything, and if he will do it, I say we ought to encourage him all in our power.

Mr. Dempster:—One reason why we should, as a society, encourage this project rather than others is about four-fifths of the money or at least a good portion of the money that is now invested in this scheme is invested by Pittsburgers. One of the patrons of our society, who has given money for its encouragement and for its library, is largely interested in it, and as our townsmen are more interested in this project than any other, we from Pittsburgh should show our appreciation of our Pittsburgh friend by giving it a vote of encouragement.

Mr. Becker:—That would simply mean that if a lot of people were interested in a scheme, from New York, the New York Engineers Society should encourage it, and if people in Baltimore invested money in a scheme, Baltimore engineers society should favor it. That would not convince me of anything. The man that invests has that privilege, of course. He does it for his own personal reasons and if he makes anything out of it is his gain, and if he loses it is his loss. I do not think we are called upon to sympathize with a man for his losses or to congratulate him on his financial success. As a society I do not think we should have anything to do with it. I am perfectly willing to encourage it as an engineering enterprise to the full extent of my ability but I do not think I can be convinced that the Engineers Society, as such, should cast a vote for such a resolution as presented here.

The matter was postponed until the next meeting in September.

THE WATER SUPPLY OF ALLEGHENY.

ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA,
PITTSBURGH, September 21st, 1886.

Society met this evening at 8 o'clock, thirty members being present. The President and Vice President being absent, Mr. Geo. H. Brown was called to the chair. After the regular routine business was transacted, the subject for discussion for the evening was announced by the President to be "The Water Supply of Allegheny, Pa." The Secretary opened the subject by reading the following letter from J. H. Harlow, President of the Water and Gas Works Construction Company of Pittsburgh:

PITTSBURGH, Sept. 20, 1886.

S. M. WICKERSHAM, ESQ.,
Sec. Engineers' Society, City.

DEAR SIR:—I was much surprised to see in yesterday morning's paper, that I was to speak on "the necessity of extensions in the Allegheny water supply," but as I shall be several hundred miles from here, I am afraid that you will be disappointed. (?) I might say, however, that while I have not sufficient data to base much of an opinion on, I am inclined to think a change in the source is only a question of a short time. My present impression is, that they should be carried up above Claremont. Whether it is wise to go farther, I am doubtful. The proper method, it seems to me, for the City of Allegheny to arrive at a correct conclusion on this question is, to employ some competent engineer, who shall give the question a thorough examination on all points. It would be probably best to employ as chief engineer of this examination a non-resident, so that he should be entirely free from any local prejudices or interests. My idea is, that the pumping machinery should remain in about its present position, and that the water should be brought to the machinery by gravity, through a conduit which shall extend up the river to the proposed source. At this point some method of filtering should be devised, so as to give Allegheny clear water. Then the water should be pumped to the reservoir located on one of our hills, and have a capacity of not much less than one hundred million (100,000,000) gallons. With such hills as we have, there is no reason why, with the proper system of distributing mains, we should not have sufficient force to throw water out in case of fire, without the intervention of steam fire engines. The fire hydrants now used by the city are of very poor design; and in fact, so poor that no town building water works now would at all consider the adoption of them.

In this I have simply given you an outline of some of my ideas, which are subject to change on further investigation.

Respectfully yours,

JAS. H. HARLOW.

MR. BROWN, Supt. of Pittsburgh Water Works: If you build a reservoir high enough to throw a sufficient stream of water without engines, then you will have a great many other difficulties to contend with. You will have a pressure in the houses entirely too great, and you will have continuously bursting pipes, and more work for the plumbers. It would be a good thing for them.

But you would have to go to the expense of laying high pressure lines to supply the hose for fire purposes, but for carrying a pressure to throw a stream sufficient to carry over the highest houses, I think would be exceedingly unwise.

But now I am very glad that we have here the Superintendent of the Allegheny Water Works (who had just entered), and I dare say he came up here to hear the discussion in regard to the water supply, and what is said about Allegheny. We would be very glad to hear from him on the subject.

MR. ARMSTRONG, Supt. of Allegheny Water Works: I do not know that I have anything to say here to-night. I came here for information, hoping to hear gentlemen present discuss this matter, and anything I can enlighten them on I shall be pleased to do so.

I do not know what position gentlemen have taken here to-night, or what has been said regarding the water supply of the city I represent, but whatever has been said, I presume it has been said intelligently. The question of the water supply is a very serious question, for it carries with it the health and sanitary condition of the city. Still there is no way that I know of that this matter can be discussed thoroughly, unless it has been thoroughly canvassed, and so far as I am personally concerned, I cannot say that I have given it that thorough canvass it ought to have.

Mr. MILLER, of Duquesne Forge: I was a little amused to hear Mr. Brown talking about the "smell" of the Allegheny water. I have lived three years in Allegheny, and have not found the water smelling yet. We have had muddy water, but not to be compared with Pittsburgh. I think it is far superior to what we have over here.

I think Mr. Armstrong can tell us, while here, that the water would be a great deal purer if they would bring it from farther up the river, a little bit farther than Pittsburgh, to get clear of the refineries, etc. I think Mr. Armstrong is quite competent to tell us how Allegheny City should get better water than they have at the present time.

Mr. ARMSTRONG: If it is a question of getting better water, it is not very hard to determine that point. As a matter of course, the farther we go up the Allegheny river the better it will be; but there is something behind all that. The question is, Are we going to do good in proportion to the expenditure? Because it will cost Allegheny City a large expenditure of money to bring the water from farther up, or from any other source than where she takes it at the present time. She certainly will have to go several miles up the Allegheny river.

So far as the impurity of the water of the Allegheny river is concerned, I have not yet had anything definite, that is, there has been no positive analysis made of the water, and consequently we are speaking merely from hearsay, an imaginary position.

Now, if it should be determined that the water is impure, then the question arises, Where are we going to get good water? As a matter of course, the farther we go up the purer the water gets, and in reading from the charter to-day, granting Allegheny City the right to build water works, I find that it also gives them the right to prevent people from contaminating the water. Even Pittsburgh would be obliged to arrange for her water supply differently. But unfortunately, Allegheny City has been contaminating the water herself, and in that case we would have to commence at home.

This matter of impure water has been talked of from time to time for several years back, and yet I have heard an old physician say, that he never knew any person to die from drinking Allegheny river water, consequently it can not be so bad; but it is a matter of analysis—of actual test.

Mr. BROWN: Mr. Miller thinks the Allegheny water is not half as muddy as our own. I would refer him to a chemist. Mr. Hunt made an analysis of both the water of Pittsburgh and Allegheny, each sample taken from hydrants. I am sorry I have not the analysis with me, but it shows twice the amount of mud, twice the amount of free ammonia, and twice the amount of poisonous matter as it contains in Pittsburgh. I am inclined to believe that Mr. Miller is not a very good judge of pure water.

Mr. ARMSTRONG: Pittsburgh has a reservoir capacity of 100,000,000 gallons (Brown, 118,000,000), while Allegheny City has but 7½ millions. Pittsburgh consumes in a day about 22,000,000 gallons, and Allegheny 15,000,000 gallons. We empty our reservoir twice every day, and you empty your reservoir every three or four days. We get water in the morning that we pump in the evening, and the water pumped in the morning, we get for tea in the evening, consequently we get all the impurities passing through the pipes of the city. You have some chance to settle in the reservoir.

Had we an equal reservoir capacity, our water would get no impurer than Pittsburgh. As to muddiness, I made an examination of the Allegheny river, and found it as far up as Tarentum as muddy as in Pittsburgh, perhaps muddier. We cannot get rid of muddy water. It is only a question of holding and settling capacity.

Mr. BROWN: It is in the water, for it certainly would not account for the presence of twice the amount of free ammonia and other impurities.

Mr. ARMSTRONG: I do not know how you are situated in Pittsburgh, but in Allegheny we have some pipes where the consumption of water is very limited, consequently the flow of water will be much less and settlings will take place, and unfortunately it may be that that water that was examined came from some such place. If you make a continuous current, of uniform velocity, the condition of the water will be alike all the time, or very near alike.

Question by a Member: Did not the chemist take the samples of water that that analysis was made from, from the pipes?

Mr. BROWN: It was taken from the hydrants. The sample of Pittsburgh water was from his own hydrant in his office, and the Allegheny taken from some store in Federal street.

Mr. ARMSTRONG: There is a matter I would like the Association to discuss, and that is regarding the economy of getting pure water for domestic purposes. It is this: Do you not think that we had better try to encourage domestic filtration—that filters would be used in the separate houses or by different families?

The question of filtering large bodies of water I would rather not touch upon, but I think it can be done successfully by small filters for domestic purposes, each house having a distinct filter. It would remove a great many of the complaints that are now made regarding the condition of the Allegheny river water, under certain circumstances. To tell the truth, I regard it as most pure when it is regarded as being most impure, so far as health is concerned. Whenever a freshet takes place, as a matter of course the banks

become overflowed, but it washes away all impurities, and we have the best kind of water in the Allegheny river, with this exception, that it is muddy. It is only a question of removing that mud and getting clear water, and that can be done, I think, very successfully by small filters.

So far as the matter of sewage is concerned, that is something that cannot be removed. That is the dangerous element in the water. But the question is, if you would consider the volume of water that extends from here to Kittanning, for instance, forty-nine miles, and that that volume of water passes here every ten hours, it must be admitted that the amount of impurities that enters into the Allegheny river must be a very small quantity in proportion to the volume of water passing out. I have oftentimes, for my own personal information, taken a skiff and crossed the Allegheny river, examining where our intake pipes are laid, and the result was that where the pipes are located the water was perfectly clear during a low stage of water. There was nothing of a discoloration of the water that I could perceive, but the closer you get the more the discoloration takes place; but I do not think that the sewage reaches our intake pipes. The only way is by a freshet. The banks being covered with refuse matter, the first flow of water carries that down and works it out to the centre of the river, but at an ordinary stage of water it does not reach our intake pipes.

The last pipe, I had the channel of the river drenched out six feet deep, but the current of the river has covered it over and has made the bed of the river as smooth as a floor. You cannot find the pipe unless you go right where it is located. I cannot say, then, that we get any of this sewage matter. It certainly keeps on the surface, and must pass over our intake pipe. If a close examination was made of the pipe of the two cities, I believe Pittsburgh would be found to be as bad as we are. The old pipes that have been laid twenty-five or thirty years ago, have been gradually getting the impurities from the water, and the orifice gets smaller and smaller. For instance, four or six inch pipes I have taken up, I have found where the cross section did not exceed over two inches, showing that the water is continuously giving out its impurities.

Mr. Brown: In regard to the filtering process, it is the correct thing. If you do not wish to extend the reservoir capacity you can adapt this system, so as to get rid of the settlings, by having each house filter its own water. Some people have asked for filtering beds, but when they would come to find out the expense in constructing them, they would be the first to have it stopped. For filtering beds for the city of Pittsburgh, it would not take less than half a million dollars to construct them, and would require from thirty thousand dollars to fifty thousand dollars per year to maintain them.

Of course these figures are only estimates. They may be very wide of the mark, but that is the best of my judgment in the matter, and I think the amount of money put into filtering beds can be far more economically used in having a more extensive reservoir capacity. I do not think there is any question as to that.

Filtering beds are never employed, so far as I know, except in small towns that require only a very limited supply. In some places where they use them, a good size house filter would filter all required.

Question by a Member: How long does it take to settle?

Mr. Brown: That depends altogether on the water, but take the Monongahela river, it will take two weeks, because it has in it that very fine silt in suspension. I do not know how it is with Allegheny, but with us the water of that river, after a freshet, requires never less than eight or nine days, when the water begins very quickly to clear up. If you had a supply, a storage supply of say ten to fifteen days, then we could pump into a different basin and let it settle. If we had thirty days' storage capacity, allowing fifteen days for each reservoir, then we would have always clear water.

Question by a Member: Would exposing the water to the air for a length of time restore its clearness?

Mr. Brown: No, not for that length of time, in such large quantities. In small quantities, I suppose it would. I do not think it would in large quantities. You take lakes, where there is no visible outlet or inlet, the water will keep pure. It always seems to be pure. It does not seem to be contaminated at all. A great many fresh water lakes must be fed by springs so as to keep up the waste by evaporation. That would be all the circulation it has.

By a Member: That would tend to show that there is a certain amount of purification going on.

Mr. Brown: Yes, I suppose there would be. That is a subject I could not speak definitely on.

On motion, the discussion was postponed and the meeting adjourned.

S. M. WICKERSHAM,
Secretary.

THE WATER SUPPLY OF ALLEGHENY.

ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA,

At the regular meeting of the Society held October 19th, 1886, Mr. Dempster in the chair, discussion was continued on "The Water Supply in Allegheny."

Mr. PHILLIPS: I do not know that I can say anything at all to throw light on the present question. I carried out some examinations of the water in the spring of 1883 and fall of 1884. The conditions were so different then from what they are now, that what was true then would not be true now.

I understand that one of the questions that come up at the last meeting was, where the source of the supply could be looked for, if not from the Allegheny river. If you take the census of population from some of the different water-sheds, as well as I remember now, the city of Rochester has the first place among the large cities, with a population of thirty-six to the square mile of water-shed. Then the next one is Pittsburgh. Taking the census of 1880, and counting the population very carefully, dividing the counties in the whole region drained by the Allegheny, I estimate it as 52.6 to the square mile. Now New York City is supplied from a region where there is a population of sixty-five to the square mile. Philadelphia has two hundred. Of course, this is not counting the population in the cities themselves, that is, Pittsburgh, Philadelphia, New York, etc. It is the population higher up the stream that would affect the water.

If the Allegheny water has so small an amount of drainage, relatively speaking, why should it not be perfectly good yet? I do not see that we need go very far to look for a source. My examination of the water led to the following result: I found that the Pittsburgh water, if the quantity of nitrogenous matter in the Pittsburgh water is represented by the number 11, that of Allegheny would be represented by the number 12. That is, there is a very slight difference in favor of Pittsburgh—12 parts of nitrogenous organic matter in the water as it comes down from the Allegheny, where there are about 11 in the water supply of Pittsburgh. These figures represent the average of seventy-seven tests of Pittsburgh water and of about one hundred and fifty tests of Allegheny City water. The Pittsburgh sewers help to add to the impurities of Allegheny City water. According to Wanklyn, drinking water should not contain much more than ten to twelve parts of albuminoid ammonia in one hundred millions.

I am afraid that I am simply going over old ground in mentioning these facts, that I am saying nothing new at all, so that you will have to excuse me; but as I said, I have not made any tests of water lately. I consider single tests as of doubtful value, because the quantities looked for are so infinitesimal as compared with ordinary chemical work. To determine one part of any substance in a hundred millions is a very delicate operation, and here in the case of the water supply it is often a question of one or two parts of nitrogenous matter in a hundred millions.

There is a more important reason, however, that is, the daily variation of the amount of such nitrogenous matter in the same water. In the Allegheny City water I have found it to vary from two parts to more than twenty parts in one hundred millions.

Mr. DAVIS: My idea of the Allegheny water is, that it is good. Very few cities have as good source of supply as is afforded by this river. Some years ago, when city engineer of Allegheny, I had occasion to give this matter some study.

In 1870 Councils appointed a committee, consisting of Water Committee, Superintendent of Water Works, and City Engineer, to examine the subject of water supply. They submitted a report (this was printed in pamphlet form) and recommended that for future supply of the city the water should be taken from Huling's eddy, at the Narrows, a point up the river some ten miles above the present pumping works. I still believe this to be the best place to draw water from. The water has better depth here than at any point below. The deep water comes in close to shore. The hills are high and precipitous. There is no room on the shore for any large collection of dwellings or manufactories to foul the water. The river water as supplied now cannot be regarded as unwholesome. It contains considerable sediment after heavy rains. This condition of water continues longer than it otherwise would on account of the interruption of the

current, imposed by the operation of the dam in the Ohio below the city. This dam makes a pool which extends at least a half mile above the pumping works.

Before the dam was erected the muddy water was soon carried past the city by the current; now it is held back, and as a consequence turbid water is supplied for a longer time.

The appearance of the water in the vicinity of the water works, when the dam has been up for any length of time, is not inviting. The light refuse and waste thrown in the stream floats on the surface, and as there is no sensible current this soon forms a considerable accumulation, especially near the shores, and gives the impression of foul water. Of course the increased depth given by the dam makes the supply comparatively free from such surface impurities, as the water can be drawn from a depth below the surface.

The contamination of water by sewage is as yet hardly appreciable, as the quantity of sewage discharged is so small compared with the volume of water running in the river; but as the districts in the valley above become more thickly settled and manufactories are extended, the river will receive a great deal more waste, and the dam, in periods of drouth, will, by retarding the flow and creating a pool, retain the sewage and seriously affect the purity of water.

The low water flow of the Allegheny is estimated at 10,000 gallons per second. The minimum flow of the two rivers, Allegheny and Monongahela, as measured in 1879, by Mr. William Martin, Assistant Engineer Davis Island Dam, is 12,267 gallons per second. The area of the drainage basin of the Allegheny is about 12,900 square miles.

The dam, at low water stages, as before mentioned, affects the purity of the Allegheny water to some extent, by making the movement of water sluggish, and by giving more time for the filth entering the river above to permeate the mass. This turbid condition of water promotes deposit also, which in course of time will be a source of annoyance.

Summer freshets will not have the same effect in scouring the river bed and cleansing the margins they otherwise would. On this account, and in view of the probability of the early construction of a dam in the Allegheny at the head of the present pool, above the water works, which will back the water above Sharpsburg, or to a point near Pittsburgh pumping station, making a second pool some six miles in length, I think it would be desirable to have the supply drawn from the point named. The valley on the right bank for some eight miles above Allegheny, will in the near future be taken up with manufactories and dwellings (the opposite bank, for the greater part of this distance is already so occupied).

There are now competing railroads in part of this valley, and there are three main gas supply lines extending through it.

The sewage of the settlements on both sides of the river will naturally be discharged into it. It is a difficult matter to prevent the streams from being made the receptacles of sewage. In England efforts have been made to prevent the pollution of streams by stringent legislation, but without avail.

It has been suggested that the water might be filtered. Artificial filtering beds were proposed for the St. Louis water works in connection with settling reservoirs at Bissel's Point, but that feature was never carried out, the settling basins doing the work very satisfactorily.

Lowell and Providence have had some experience with filtering galleries. In both cases they have proved unsatisfactory. Newark used a natural filtering bed, but it did not work well; the fine sand and clay soon silted up the gravel, and made the filter inoperative.

In the neighborhood of the point proposed for source of supply and pumping works, there is favorable ground for reservoir purposes.

The storage and settling reservoir should have a capacity of eighty or one hundred million gallons, so as to give four or five days' supply to a population of one hundred and fifty thousand.

This reservoir should be placed at an elevation of some two hundred and sixty or two hundred and eighty feet above the river, so that the water after settlement could be delivered to the Troy Hill basin. This latter should be enlarged to double its present capacity, or to hold at least eighteen million gallons. This would give a good distributing reservoir in the heart of the distribution, at an elevation of some two hundred and twenty feet above the river.

This basin will give a good head of water, without causing extra strain on the plumbing, in the thickly built up parts of the city. It commands a district of about three and a half square miles, or one-half of the present city area. The enlargement can be made by extending the reservoir out to the point of the hill and raising the flow line. The slopes of hill should be protected and the reservoir grounds beautified.

Above that level there should be separate pumping works similar to those now used on some of the hills; these are isolated points, and should be treated independently.

Mr. Harlow mentioned in connection with filtering water, that the supply should be brought to present works by conduit and then pumped to a large reservoir located on one of our hills. In my examination of grounds for reservoir sites, I found very few places suitable.

The elevations of hills back of city range from four hundred to five hundred and fifty and even to six hun-

dred feet, and most of these are a long distance back from river. The intervening ravines or depressions, where they reach the proper elevation, are too broad, or are objectionable in other ways, to take advantage of for reservoir sites, as has been done elsewhere, notably at Cincinnati.

Reineman's Hill gives a good location in some respects, though it has the objection of excessive height, and the further objection of being, in part, honeycombed in mining for coal. Pittsburgh has had some experience in this line, I believe, at Herron Hill reservoir.

The delivery main from the reservoir up the river to the Troy Hill reservoir would supply the settlements along the line above the city. The route for this main would be, preferably, by the line of the West Penn. Railroad.

A supply can be obtained from the head waters of the Kiskeminetas and Conemaugh, but it would be at a cost disproportionate to its advantages; there is fall enough, and by the use of large storage reservoirs at the sources, excellent water can be obtained.

Such a project will not likely be seriously considered soon. I think we have good water near us in the Allegheny. We have only to go above the thickly settled districts and above the proposed pool to reach it.

As a sanitary measure, the local sewage, or the bulk of it at least, should be intercepted and carried to a point beyond the limits of the city, below Davis Island dam.

This is especially necessary now since the dam is in operation, as the river, during the period of low water, is made a sluggish pool.

The small streams discharging into the river in vicinity of present pumping works, and above on both sides of the river, are used as sewers.

Mr. Danse: I rise for information. I would like to ask the gentleman what the distance is to Huling's eddy?

Mr. Davis: About ten miles, or scarcely ten miles. I would like any of the gentleman who are visiting up that way to inspect this place. I think it the proper place, and you would be pleased with the location.

Mr. Scaife: I noticed in the report of the last meeting, that the Superintendent of the Allegheny Water Works had said that he believed the Allegheny water was purest when it was muddiest. If I understood Mr. Phillips aright in conversation with him today, he said that in his chemical examination of the water he found more nitrogenous matter in it when the river was high than when it was low.

Moreover, Mr. Davis has expressed the idea that when the wickets of the Davis Island dam are up, the water is not as pure as when they are down, owing to the back water. Now, if the muddy water drags along nitrogenous substances, and therefore becomes more impure, will not the river water be purer when the dam is raised and a settling is going on in the back water, than it would be in a strong current?

Mr. Phillips: I found that with very muddy water there was about five times as much nitrogenous matter as in the clearer water. That was specially at the time of the great flood in 1884—in February or the beginning of March, 1884.

I have found that the water, when so low as to be below the lowest mark at the bridge, was very pure. It did not look so, but it was very pure, purer than when it was very high. In fact the amount of impurity is in direct proportion to the rainfall and depth of water. The deeper the water the less the purity.

The less the current the less the impurities, the greater the current the greater the impurities. It was so when I made my tests, in June, and was so during October, November and December, of the same year, 1883.

Mr. Dempster: Mr. Phillips, how do you account for that, was it an eddy?

Mr. Phillips: The water was taken from a hydrant in the city, on North Avenue. It only shows that when there are floods, more of the general drainage is washed down.

Mr. Danse: Don't you think that was due to the fact that where the water was very shallow there was greater opportunity for oxidation?

Mr. Phillips: Yes, I think it was (although I do not know that that would fully account for it), because when the water is running very fast, the movement of eddies should promote oxidation.

Mr. Wickersham: It was once said, in the presence of Benjamin Franklin, that if a tub was filled with water and a ten pound fish put in, it would not weigh any more, and Franklin was asked why it was so. Well, he said, before I give a decision on that point, I would like to see the experiment tried. I would ask the gentlemen if there is any instance of any person in Allegheny, having been poisoned or made sick by the impurity of the water, that is through any poison supplied through the hydrants. I never heard of a case of that kind, and I don't think a case has ever occurred unless through local causes in the pipes. Water may stand for a long time in a lead pipe and gradually, not being drained off, may take up some of the deleterious matter from the lead and may make a little sickness, but the people have never been injured by taking water from our river. I have no evidence of it.

It was said at our last meeting that the water after a freshet was purer than after a long drouth, and I think it is very reasonable to think it may be so, for the very current will remove impurities that otherwise would be in the water. And I can readily conceive that muddy water after it has settled a little

may be better than before it was muddied. I do not think it is a hard thing to conceive of it at all.

Then another thing was said at our last meeting, that the influent pipes were placed in the middle of the river, that in times of high water the water that runs down, all runs over the influent pipes, that the impurities were carried over it and none sunk down, and in times of low water these things all kept near the shore.

Mr. Armstrong told us that the water, in very low water, was good at the influent pipe, and clear. Now, if that is so, it seems to me the whole question is narrowed down to, where is the best place to get water with the least impurities? It is a matter of importance because it concerns the health of the people. If you can get above the source of impurity, so much the better.

In Allegheny we have to look forward to supplying a much larger district than at present. Our line will soon embrace Sharpsburg on the one river, and I do not know how far down the river, but I think to Lowery's run. It is likely we shall have to supply in a few years a population of at least 150,000 people, and we need some more extensive supply than we have now; but when we think of the immense amount of water that flows down the Allegheny river, and compare it with the quantity required for the city's use, we need not fear any bad results from the impurities which find their way into it; but an increased quantity we must have, and we ought to go for it, to whatever point required, where it is now uncontaminated and not likely to be for some time to come.

Mr. A. E. Hunt: The analyses referred to by Mr. Brown, in the paper that was sent out of the transactions of the last meeting, were relative to the comparative purity of the water furnished through the city conduits of Pittsburgh and Allegheny City. The samples from which the analyses were made, were taken on August 6th, 1886; the one at 32 Federal street, Allegheny City, and the other at our laboratory, at 98 Fourth avenue, Pittsburgh.

Before discussing the relative merits of the two samples, I will first explain what the measures of impurity in samples of water really are.

Chemists analyze suspected waters for free ammonia, albuminoid ammonia and chlorine, as indicating, or rather as evidence of impurity. I say evidence of impurities because these chemical substances are not the impurities, are not the poisonous elements in the water.

I have read quite a number of articles lately, in which the writers expatiate learnedly on various water analyses, and talk of the poisonous and deadly amounts of ammonia and chlorine in the waters. These are not the poisons that are in the water—they are simply the evidences that show how much poison, how much sewage, how much animal or vegetable refuse in a state of putrescent fermentation is in solution in the water, and with this explanation, I would claim that the analyses of the Pittsburgh and Allegheny waters, neither of them show an alarming state of affairs, but rather a comparative freedom from impurities. I make this statement qualifiedly, that is, that *these analyses* show the comparative freedom from impurities; however, I certainly agree with Prof. Phillips, that these two analyses are too few to generalize on; but I would, however, take exceptions to the inference which is implied by the professor—that the almost infinitesimal amounts, as in the few parts per million, is very difficult to determine analytically, for the wonderfully delicate test of Nessler has made these determinations, in the hands of a reasonably skillful analyst, certain and reliable.

The following are the results of the analyses, placed side by side for comparison:

	Allegheny Water. Parts per million.	Pittsburgh Water. Parts per million.
Free Ammonia	.05	.02
Albuminoid Ammonia	.03	.04
Chlorine	13.00	12.00
Total Solids	137.50	87.30
Silica	32.10	13.80
Oxide of Iron	2.10	1.80
Carbonate of Lime	40.00	30.00
Sulphate of Magnesia	5.00	5.50
Carbonate of Magnesia	15.40	8.10
Carbonate of Soda & Potash.	21.50	8.40
Chloride of Soda & Potash	21.40	19.70

Before I sit down I would call the attention of the members of the Society to something that ought to be of interest to us all, and that is now being worked up by eminent chemists. What has been found out by experts so far, is, that the ammonia indicates sewage, and that the sewage has in it minute germ growths that can be seen by a microscope. It is a question not yet settled by chemists and biologists, whether these germs are a vegetable or animal growth, but they are similar to the germs that cause fermentation—you know that there are several kinds of fermentation, one of them is the alcoholic, requiring the yeast germs to produce it in saccharine solutions; this kind of fermentation I need not say anything more about to the members of this Society, as many of them have experimented with it in many ways. The second is the acetic fermentation, as in making vinegar with its superinducing germs, existing in the gelatinous substance, the housewife calls the "mother;" another form is what is called the putrescent. In any one of these forms of fermentation there needs to be a germ or spore. It is a little organism that is needed to cause

the fermentation to start. It is something that sometimes exists in the air, that finds a fertile home in water fouled by impurities, and is transmitted in many ways; and it is the cause of the fermentation.

Pasteur says that it is the inoculation of the system with these germs that causes rabies in dogs, that in a varied form causes typhoid fever. He says that in the blood there is something of the same form going on in consumption, and he believes that many of the forms of disease, which man is cursed with, are really due to these minute organism, which exist in nature, in putrescent matter.

Certain it is, that the particular poisonous ingredients in water, which we are now discussing, are not the ammonia salts nor the chloride of soda, which are harmless; it is not the free ammonia, or the albuminoid ammonia, but it lies in these things which the chemists cannot get into such shape as to be able to separate and determine chemically; so that I have a further reason for qualifying my statement that the analyses given above do not condemn the samples of water for portable purposes, for I do not believe that the chemical analyses alone should be taken as indicative of the purity of the water, but simply as an indication of its character.

Mr. SCAIFE: I am glad that Mr. Hunt has brought up that last point. I was about to ask a question in regard to it. Should we not look beyond salts and mere chemicals, as poisons in the water, to the organic germs it may contain?

Some years ago, I visited a meteorological observatory in Paris. The gentleman in charge showed me an arrangement for collecting the germs floating in the air. He said that the air at all times contained germs in greater or less numbers, and that he could tell the general condition of health in the city by the germs which came floating to the observatory. They were first examined under the microscope; then subjected to a kind of culture in order to develop them, and to show what they would grow to. After that it was proposed to inoculate animals with them, and by watching the results, to determine their probable influence upon the health of the people; and finally, to learn by experiment what would destroy the germs.

As our river waters are likely to contain similar germs, may not their presence account for a portion, at least, of these nitrogenous substances? To determine the quantity of the latter is the province of the chemist; but then, should not the physician step in with his microscope and examine the germs themselves, subject them to culture, and find what effect they may have on the human system? If that were done, perhaps some remedy might be discovered to counteract them, so as to prevent and cure diseases resulting from them. The absence of such germs might also account for the fact which Prof. Phillips mentioned to me to-day, that epidemics do not always occur when the water is bad—that is, when chemical examination shows the water to be bad, according to the standards that have been given, wherein their presence might produce much sickness in water chemically potable. I think, therefore, it might be well for our chemists to take competent physicians into partnership; and when called upon for an analysis of water, as to its hygienic properties, to give not only a chemical analysis, but also what might be called a biological analysis.

Mr. PHILLIPS: I think Mr. Scaife is exactly right, but one difficulty is the quantity and variety of these organisms, so great that no one can define their effects. In connection with the work of the London Commission on the Thames water, they have employed some of the first biologists in England, among them Dr. Lancaster, who stated that he found, I think, over thirty-two to thirty-eight different kinds in a single sample of water, and that is nothing unusual. Many of them are known to be absolutely harmless. The great difficulty with the chemical methods of determining the nitrogenous matter, would make this all the more desirable if it could be done, because the nitrogenous matter of the Allegheny water will vary from day to day, and from two parts in a hundred million to twenty, which is an enormous variation, so great that a biological method of examination would be very desirable. At present the biologists are not prepared to settle the question as to the safety of drinking water, although it is desirable to know something about, and we must fall back upon the chemical methods.

Mr. DEMPSTER: I am a little practical. We have the Allegheny river water—that is the best we have and all we have. The question will be, however, where, in the Allegheny river, is the best place to get pure water—the best and cheapest. Now, if I were a member of Allegheny Councils I would look at both those elements. The water up about Claremont is as good as any place on the river. I have seen the place, and I think a very economical arrangement can be made by making use of the unfortunates who go up to Claremont, and with their aid the citizens of Allegheny City could find no better way.

The question as to the purity of water in the time of freshets or in currents—the fact is, that currents over pebbly beds will purify the water, and that stagnation will hold the water and will hold the impurities in the water. In times of floods the impure matter is washed down from the banks of every little stream and is carried with the current, and the amount of impure matter will be at maximum; while in the time of low water there is very little in it. The water from the

head waters is pure, because in coming down it flows over pebbles, as you can learn by taking water from above and below a riffle.

Mr. Danse gave some experience and some data he had gathered while boating on the Allegheny, to the effect that the water was very much better at some times than at others, and that he had seen no perceptible coloring or trace of impurity at the mouth of the influent pipes; after which the Society adjourned.

THE WATER SUPPLY OF ALLEGHENY.

NOVEMBER 16TH, 1886.

The Engineers' Society of Western Pennsylvania, E. B. Taylor, President, in the chair, met, and after their routine business, continued the discussion of "The Water Supply of Allegheny." Mr. T. P. Roberts, Chief Engineer of the Monongahela Navigation Company, read the following paper:

THE OXYGEN TEST.

When is Water Unfit to Drink?

The following brief paper, by Prof. Albert R. Leeds, I have never seen elsewhere in print, save in the report of the Philadelphia Water Department for 1875. In the belief that it will prove interesting to those who have not seen it, I venture to read it here. A careful study of it, in connection with Prof. Phillips' able paper read before our society some time ago, supplemented by his remarks and those of Prof. Hunt at our last meeting, will, I am sure, assist in allaying the feeling of alarm manifested by many citizens in regard to the character of the water in the Allegheny river. That stream, I am quite convinced, is to-day, six or seven miles above its mouth, as pure and wholesome as when Washington stood on the site of the future Fort Duquesne, and pronounced it a "good place for a fort."

"There is perhaps no question more important to the inhabitants of many cities, nor one which more severely taxes the resources of applied science, than the determination of the fitness or unfitness of a water supply. The difficulty arises from the fact that, in some cases, a water may have taste, smell, color, and a considerable amount of foreign matter, and at the same time be drank with little or no injury;* while another water, which is agreeable to the taste, limpid, colorless, and with little foreign matter, may yet contain abundant sources of disease.

"The literature of the subject shows that there are two classes of thinkers, one of which puts great faith in the efficacy of natural agencies to bring about the purification of polluted streams; the other which contends that the only safe plan is to reject water which has ever been contaminated by sewage, etc. The evidence elicited by the Royal Commission on the water supply of London is that principally quoted by both classes, and cannot be regarded as conclusive. The rapid extension of our knowledge in this branch of sanitary chemistry is such, however, that we may anticipate greater certainty in these matters, and imparts great interest to some recently published methods of investigation. Any one who refers to analyses made a few years back, will find that it was deemed sufficient to give the character and amount of the mineral substances contained in the water, while the organic and volatile substances were expressed in a sum total, no attempt being made to determine their precise character. But, except in cases where the mineral substances were positively deleterious or excessive in quantity, this did not settle the question. Of late, the greatest attention has been paid to the organic constituents, and the analyses state what amount of putrefiable matter is present. A careful determination is also made of the amount of ammonia and of nitrous and nitric acids. These are regarded as the forms which the organic matter in large part assumes after it has passed through the putrefiable stage, and indicate, therefore, the degree of previous contamination.

"But it is said, and with truth, that all these things may be known to a wonderful degree of nicety, and yet there may be substances present capable of rendering the water altogether unsafe for drinking. It is urged that the living organism is exceedingly sensitive to substances whose capacity for injury is fatal, even when present in amount so small as to render their weighing, and even detection, impossible. But of late the fauna and flora of water courses have been studied, with a view of learning what assistance they could be in the matter, and the results are highly encouraging.*

"It has long been known that *dissolved oxygen* played

*Allegheny City draws its water supply from a point less than a mile above the mouth of the river. At times, it is possible, some of the manufacturing refuse, etc., from sewers on that side of the river, above this point, may reach the influent pipe, Hence the complaint of the "smell" to the water, sometimes heard, etc. Others, however, drawing from different hydrants, will deny that there is any such smell. Both may be right. To settle this point in dispute, regard must be had FIRST to the *time* in which the water has had to ferment in the pipes; SECOND, to the difference in temperature of the respective hydrants. Thus I know of hydrants in Pittsburgh, probably near "dead ends" little drawn upon, with the pipes laid deep, or in the shade more than usual, which furnish potable water in the hottest days of summer. On the route of mains largely drawn upon, the earth surrounding the pipes late in the summer approximates in temperature that of the water in the river. The pipe lines are certainly distributors of heat. T. P. R.

*Profs. Phillips and Hunt both refer to the value of microscopic research in conjunction with chemical analysis in the investigation of drinking waters.

a great part in the purification of streams, and was the principal agent by which putrefiable substances were broken up and were converted into harmless inorganic compounds. A recent essay by M. Geradin, to which the prize was awarded by the Paris Academy of Sciences, contains some striking results obtained by the above mentioned methods of investigation. These methods, to summarize, were:

"1. A determination of the amount of oxygen held in solution.

"2. An observation of green plants and aquatic mollusks.

"3. A microscopic examination of algæ and infusoria.

"It is claimed that the results obtained by these three methods were identical, and that, where the water was clear, with abundance of fish, water-cress, etc., the water contained a correspondingly large amount of oxygen; while in places where the dissolved oxygen was small, fish and the higher types of aquatic plants were wanting, and certain low forms of vegetable growth had taken their place. The river Vesle, in France, from Rheims to Braisne, was taken as the field of observation. It was studied over a distance of thirty-seven and a half miles, during which it received the sewage of one large town (that of Rheims, the daily flow of which amounts to 4,180,000 gallons), and other impurities. Above Rheims, the water (which was clear, wholesome, and with abundance of fish, charas, water-cress, ivis, etc.,) contained 0.66 cubic inch oxygen in sixty-one cubic inches of water. In passing through a suburb above Rheims, the Vesle received the refuse of some dye works, which colored the water, and in place of the fish and water-cress, *sparganium simplex* makes its appearance. At a point where the water had received the contents of the five principal sewers of Rheims, the water was thoroughly polluted and contained but 0.03 cubic inch of oxygen in sixty-one cubic inches. Two species of *algæ*, the *biggiatoa alba* and the *oscillaria nataus*, were developed largely, the latter to such an extent that the whole surface of the sluggish water was covered with a thick blackish coat.

"Above the mill at Macan, where the oxygen had increased to 0.45 cubic inches, the two varieties *algæ* above mentioned had disappeared, and the bed of the Vesle was covered with a long whitish *alga* called *hyphœthrix*.

"At Compensé mill the oxygen had increased to 0.5 cubic inch, the *hyphœthrix* had almost completely disappeared, and the *sparganium simplex* was again abundant. Below this point, the amount of oxygen increased, and with it a corresponding change took place in the vegetation, until at Braisne the water contained 0.66 cubic inch of oxygen per litre, all traces of pollution had disappeared, and fish and water cress flourished.*

"From this it would appear that a properly areated and pure water showed, when polluted, the amount of pollution by a corresponding diminution of oxygen, by the appearance of *sparganium simplex*, *spirogyra*, *hyphœthrix*, *biggiatoa* and *ossilaria*, and progressive improvement by a corresponding increase of oxygen, and the appearance of these plants in the reverse order. It remains for us to apply and extend this knowledge to our own streams. Fortunately, the means are not wanting, since the great monograph on the fresh water *algæ*, magnificently illustrated with plates, by Dr. H. C. Flood, which was not published by the American Philosophical Society, has been recently printed by the Smithsonian Institute."

I have for ten years, in the course of travel, and while engaged in surveys along streams, kept in mind the oxygen test for the purity of their waters, by observation on their fish life—necessarily confining myself to this division of the subject for want of the facilities and time to devote to the vegetable and microscopic organisms in the water, but I would commend these departments to those who have the time and the instruments, as promising fields for the development of information of the greatest practical value to the people of our country.

Our country is so rapidly being populated and covered with great cities and towns, that the question of a pure water supply for them demands in many cases the application of all our means of knowledge. Too often the question of the source of water supply is left to engineers for settlement, whose sole care is that the pipes be large and strong enough, the reservoirs tight, and the engines equal to every demand—and it is a great thing to know how to do all this.

Allied with his practical knowledge, the hydraulic engineer should acquaint himself with every known means to guard the health of the community he is called upon to serve; and if he feels himself deficient in any respect, should not hesitate to call in the aid of the specialists. "All things work together for the advancement of knowledge," and our profession at large is doing yeoman's service in the good cause.

These remarks have little to do with the fish test question, but they seem to be justified by the times which demand a definition of the duties and position of the hydraulic engineer, whom it is generally conceded, occupies the topmost rung of the broad and high ladder of engineering science.

The fish in the Allegheny river afford, I think, an exemplification of M. Geradin's theory, for they appear

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*It is unfortunate that distances from point to point, original volume of streams, and volume of tributaries, if any, are not here given. T. P. R.

to be competent judges of its purity as far as concerns their own welfare, as I will proceed to show.

During the years 1878-9 I conducted surveys and works for the improvement of this stream for the government, under Col. Merrill, U. S. Corps of Engineers, which included all the distance between Pittsburgh and Olean, N. Y. 255 miles. It is to be recollected that at Pittsburgh this river drains approximately 13,000 square miles of comparatively sparsely populated country, not a single town upon it possessing a sewage system, and that in its lowest stages it discharges not less than 500,000 gallons per minute.* It has about four times the area of water-shed and discharge that the Schuylkill has, and as compared with that stream receives certainly not one-twentieth of the amount of sewage matter above our city limits. We should say, therefore, having regard to these facts, that it is contaminated to the extent of only one-eightieth of that stream, if indeed so much. If, thus, the Schuylkill still furnishes water not yet proved to be unwholesome at Philadelphia (though there are grounds for doubt on this point), we need have no fear concerning the purity of the Allegheny as a source of water supply for generations to come. I would say this even if the stream was slack-watered. Philadelphia draws her supply from a dam. No river of its size in the world has ever been dangerously contaminated by sewage matter. The slight indications of ammonia found in it by Profs. Phillips and Hunt, from samples gathered from a point suspected of being bad, could doubtless be found in nearly equal proportions at its head-waters, the product of the decay of pure forms of vegetable matter.†

Yet notwithstanding the purity of the river, the fish in its waters seem to be gradually disappearing over certain areas of its course in a peculiar manner, but in a way easily to be explained, and preventable by the exercise of a little backbone in our Legislature.

Thus, for a long distance below Olean we found but few game fish; thence for some distance above Oil City they appeared to be more plenty, and were at their maximum just above that point. Below Oil City for a distance of seventy-five miles not a single bass or salmon was to be found; gradually, however, they began to make their appearance again, and just above Freeport, 100 miles below Oil City, they were seen in astonishing numbers as though penned up in the pools (unfortunately for us they would seldom offer to take our minnow bait). At Freeport they again suddenly disappeared, and were just beginning to be seen again twenty miles below when we crossed the city line, and then they disappeared again.

There are oil refineries at Olean, Oil City and Freeport, and the refuse acid from these establishments clearly accounts for the entire trouble in the fish family. We observed that the habitat of common fish, such as sun fish and suckers, was nearer to the source of contamination than the high mettled salmon and bass, and that the so-called alligator—fish or reptile, which ever it is—could exist close to the establishments. I have no doubt they would flourish in a sewer, for they seem to have wonderful vitality, and probably consume little oxygen. During all the time I was engaged on this work the river was low, and I could frequently see the bottom of the river in the long, deep pools where it was sixteen feet deep.

I mention these Allegheny fish merely to illustrate the possible value of fish in determining the constituency of the water. The chemist takes but a gallon or two for analysis; but the fish spend their entire lives in it, swimming miles daily and breathing it continually, and to them its percentage of free oxygen is of the most vital importance. Human invalids experience a benefit very frequently by "a change of air," so called, though possibly no chemical analysis would show that in moving from place to place the constituents of the atmosphere they breathe had changed, still we cannot assert confidently that the air is the same at two points of equal altitude, etc. The lungs very likely are capable of noting differences too delicate for the chemist's measure, so in the case of our fish, it is problematical whether even five miles below Oil City the most careful analysis would have indicated the presence of any sulphuric acid in the water.

Several years ago, upon the starting up of the coal washers after a period of idleness, on Jacob's creek, a branch of the Youghiogheny river, the discharge of pyritous water into that stream caused the precipitate retreat of all the fish, many thousands dying as though actually poisoned—I will not undertake to say whether they were poisoned, or simply asphyxiated Be this as it may, the water of the Yough though visibly discolored in this instance, for ten miles below the mouth of Jacob's creek, was not injurious to human beings or cattle. However, on this point of acid in our rivers, I can mention that it is noticeable in the pools of the Monongahela Navigation, that the iron parts of lock gates below water surface oxidize more rapidly now than formerly, when the coal mines were not so numerous, and also, that this effect is more noticeable in the lower than in the upper dams.

Acid instead of being injurious to us in our drinking water, may be really beneficial. This argument should not be taken as an endorsement of a too free indulgence in "Old Monongahela."

*Several years may pass before the river reaches so small a discharge.

†When a man thinks his water smells and tastes bad, it does do it so far as he is concerned. We are the creatures of imagination more than we can readily believe. On moral grounds, therefore, it would be safe—if no other presented—to urge the removal of the Allegheny water works higher up the river.

But to return to the fish—if they are such expert judges of the acidulation of water, they are no doubt equally exact analysts of its degree of oxygen at times. Is it not possible, also, that sometimes the acids from coal mines absorb the oxygen of the rivers to such an extent as to suffocate them?

And on the general merits of M. Geradin's proposition, can oxygen be in normal proportion in water, in the presence of vegetable or animal impurities? It would appear to me the irresistible tendency of oxygen to combine with them, and form innocuous salts, or mineral compounds, would make this impossible. If Geradin is right, it would follow that we have no more certain and positive test of the purity of the water in any stream, than can be afforded by a study of its animal and vegetable life.

Mr. Phillips: Certainly oxygen in the water would be a condition in the life of fish; there is no doubt about that. There is a fixed normal quantity of oxygen held in solution in water under given conditions of pressure and temperature. The ratio of oxygen to nitrogen is constant only in pure water, however. In one hundred parts (volumes) of air dissolved in water the oxygen is 36 per cent. the remainder nitrogen. This is true of pure water exposed to air.

They have made tests as to this on the Thames. Wanklyn records results obtained there. Samples of Thames water were taken from different parts of the river. It was found that above the city the proportion of oxygen was about the normal. When they came within the city limits of London, the oxygen diminished and gave place to carbonic acid. The carbonic acid was the product of the oxidation of the organic matters of the water; that is, London sewage. Going down below the city the proportion of carbonic acid increased and the oxygen fell off almost to nothing. Going still further along the stream, they found that the oxygen increased again and the carbonic acid diminished, so that in the main the proportion of oxygen was a very curious and accurate measure of the degree of purity. Where the oxygen was the greatest in quantity there was the least amount of carbonic acid.

But in this case, as in a great many others, the true method has not yet been devised. No one can exactly fix the limit of safety. I have not heard of any tests being made, based on this method, very recently, except those that Mr. Roberts speaks of. They found, as he said, in the case of London, that the great difficulty was to fix the limit of safety. While it could be safely said that water that contains the normal quantity of oxygen is perfectly safe, and water that contains no oxygen at all is absolutely dangerous, because in that case it contains more organic matter than is enough to destroy life, still no one can say just where the safe limit is between these two extremes.

Mr. Roberts: I would like to call Prof. Phillips' attention more particularly to the fish. You are speaking of the proportion of oxygen found by chemical analysis. Now, I think a good deal of what the fish find. They seem to get away very soon from water that is contaminated. Can we not learn something by observation of their actions?

Mr. Phillips: That, very likely, is a safe criterion. There is no doubt that the higher forms of life, like fish, are a very sure indication that there must be an abundant supply of oxygen there. It is well known that fish come up to the surface to breathe if the water is deprived of much of its oxygen.

Mr Roberts: It occurred to me that this suggestion was a very simple way, from observation, to determine the purity of water. The fish can easily be seen and examined, and it will not require very much skill. Of course we can go further than that with the aid of the microscope, and investigate the lower forms of life, and thereby probably arrive at some point where one can say the water is safe or unsafe. Swimming over the whole river, as they do, they necessarily must test it as a whole, whereas the chemist examines only a small sample.

Mr. Phillips: There is one reason for believing in what Mr. Roberts says—that it is known that the solubility of the gases in water varies with the amount of salt dissolved, and it has been supposed that one reason why fresh water fish are unable to live in salt water is, that the oxygen dissolved there is less. I do not know whether that has ever been demonstrated or not, but that is the supposition. It is not the saltness, but the diminished oxygen.

Mr. Taylor: These tests of the Allegheny water—were they made in the winter season or summer season, or both?

Mr. Phillips: The tests I made were in May and June, then in October, November and December, every day.

Mr. Taylor: Were any tests made while the river was frozen over?

Mr. Phillips: There was a great deal of ice there, but not entirely closed.

Mr. Roberts: In regard to the ice, I have heard it stated that that was the only time when we could taste the oil, from the oil regions. When the river was frozen over the oil running underneath the ice would be "churned" up and would be mechanically mixed with the water, whereas at other seasons it would flow on the surface. I doubt if you could ever get any taste of oil in the summer time. The refuse from the refineries is very heavy and sinks like gas tar to the bottom, and when our water works were further down the river, distinct traces of it were frequently observable in the pipes. This stuff has done thousands of dollars worth of damage to

rafts and boats, but we don't see so much of it in our rivers in late years.

MR. MILLER: I would like to know from Mr. Roberts or Prof. Phillips, if they have made experiments or tested the water below the city, below the two cities, and if so, what was the condition or difference from the water above the cities.

MR. PHILLIPS: I have made no tests.

MR. MILLER: Judging from the description of the test on the Thames, I inquired to know what was the condition of our rivers after they formed the Ohio.

MR. ROBERTS: I can tell you, Mr. Miller. I was talking to Mr. Martin on that point some two months ago. I was down at the Davis Island Dam, and I noticed he had a pump arranged to supply the house. On my asking regarding the water, he said it was very foul near the shore, and was very offensive. It looked perfectly clear, but he said it would not do at all for drinking. But I do not think the whole river was in that condition, because I have noticed the Allegheny water distinct from that from the Monongahela. You can recognize it distinctly at some stages until it has reached a point twenty-five miles below the city. The shore water from Allegheny city, with its impurities, would keep on probably for a number of miles below Davis Island Dam, but in the middle of the river I think no one would notice it. Steamboat men do not complain of the water. The people of Wheeling drink the river water, so do those in Cincinnati, etc.

MR. TAYLOR: How are the fish below?

MR. ROBERTS: In regard to the fish, I have seen seines drawn at the foot of the island at McKee's Rocks, four miles down. That was fifteen years ago, during the height of the oil refining business in this city; salmon and bass could be caught in no other way at that time. There are very few now. These salmon and bass very likely have passed the city during times of high water. They sometimes migrate, though not regular migratory fish, but move from one pool to another if they get scared, and may go miles, and they could easily find opportunity to get past the city here without injury.

I have never thought that the center of the Allegheny river was foul, although Prof. Phillips mentioned in his paper at last meeting, that the Pittsburgh sewers slightly contaminated the water supply of Allegheny City. But I have observed this fact, living on a steamboat for four years, when I was in the government service, and being up along the Allegheny in low water, that in a very sluggish current the sewage matter remained near the shores. I have noticed running from the sewers an inky black water, that would come along the Pittsburgh side and out for a distance of 200 or sometimes nearly 300 feet, so that with the sewage from both cities it would make each side of the river inky black; still a "blue streak" fully 100 yards wide remained down the center all the time. For this reason I would never believe that sewage matter from any Pittsburgh sewer could reach the other side of the river. To do it the current of the sewer would have to cross-thread a flowing river 1000 feet wide.

MR. DAVIS: Mr. Roberts considers that fish will only live in pure water. I recollect hearing Mr. C. B. Brush, Superintendent of the Hackensack Water Co. give his experience with their water in 1884 and 1885. It was at the annual convention of the American Society of Civil Engineers, at Deer Park. They draw their supply from the Hackensack, pumping it about fourteen miles through an iron pipe to a reservoir at Hoboken. There is also a larger reservoir about three miles from the source. In the summer of 1884 the people began to complain of the water, and as the season advanced it kept getting worse.

He mentioned that these reservoirs were full of fish, well stocked with carp and bass, and that fish were removed from time to time as they became too numerous. It was ascertained that the contamination of the water was from a vegetable growth. This had no injurious effect on the fish, though the people could not use the water.

This water from the Hackensack is drained from an area of about 100 square miles, and there was supposed to be no sewage contamination. It got, towards the latter part of the summer, green; a green scum formed. Then I think they put air compressors on the river bank and pumped air into the mains, thinking that would remedy the matter. For a time it did seem to improve it, then the scum would appear again and for a month would stay, and then disappear. In the following summer they had the same trouble. Some thought if the reservoirs were covered it would make a difference. He answered that by saying, they had one of the reservoirs covered and it was just as bad.

MR. ROBERTS: What was the depth of the water in the reservoir?

MR. DAVIS: The large one about sixteen feet, and was kept that depth all the time. In reference to the fish in the Ohio, I may state I have had occasion several times during the past summer to cross the river down about Sewickley, twelve miles below Pittsburgh, and old fishermen there say the fish are returning; that they now get good fish. They told me they could catch bass and perch in large numbers. They attributed the change to the absence of the refuse of the refineries, the oil and waste products of oil formerly discharged in the rivers.

MR. PHILLIPS: In that connection there is one matter of interest. The water may be unfit to drink and yet fish may live in it. There is a peculiar fresh water sponge that often forms in the reservoirs and gives

the water a cucumber taste. It was observed in the drinking water of Boston a few years ago, and it happened to a good many towns of New England, and no one could account for it. They had analyses made, and determined all the fixed solids, the ammonia and so on, but all this went for nothing. The analyses seemed to show that the water was good, and yet it had that cucumber taste and was perfectly unbearable. The doctors thought in some places it was causing sickness, although they did not know positively. At last it was found out that this fresh water sponge was growing in large quantities in the water, and when they cleaned out this sponge the water became good. Now there is an instance where water may be unsafe to drink, at least unfit to drink, from the taste of the branches of this sponge, and yet it did not affect the fish at all. It was stated that the water was perfectly good in every other particular. They had spent so many years in studying it, and so much money had been appropriated that it was looked upon as a problem that could not be solved; but Ransom of Baltimore discovered the whole trouble was due to these sponges.

Mr. Davis: This Hackensack water had the cucumber taste. In Rochester, N. Y., in 1876, the water was affected the same way, and in Philadelphia in 1883.

Mr. Roberts: Mr. William J. McAlpine, two years ago, was called upon by a company in New York to propose a new scheme of water works to supply that city. He recommended the Ramapo district in southern New York, on the Jersey line. I have forgotten how many square miles there were, but there are a number of small lakes in the district. In his report he refers to these lakes as being at different elevations, and at certain seasons, according to elevation and temperature, some of these lakes would have this spongillæ and other ephemeral life forms. His idea was to use the water in one lake while the spongillæ, etc. would be dying in the lower lake, say, and by that time the water in the other lakes would be clear. The Allegheny river is not afflicted with any such disease.

However, I can not but return to my position that the merits of this question of the purity of the water is decided by the fish.

Mr. Scaife: In this connection there is one point I have been waiting to hear touched upon, but no one has mentioned it. It is that of purifying the sewage water before putting it into the river again. It seems to me that this is a question that must be solved where the population is very dense.

Some years ago in Paris, my attention was called to this matter. The people below Paris complained very much of the sewage that was discharged into the river. They said it produced sickness in the towns lying along the river. Reports on the subject were made by the engineers, and finally, it was concluded to purify the sewage water before putting it into the river. At considerable expense a pumping station was formed, and a large part of the sewage (not all of it, because they could not handle it all) was pumped to a place where there was a large tract of sandy soil, of no use at that time. This sewage was put into reservoirs, and from there it was given to the people of that neighborhood, or anyone who wished to have it, to distribute over the land. The soil was prepared by the farmers, who made channels and furrows through it with a plow. The water was allowed to run in these furrows and to soak into the ground, fertilizing it. At the time I visited the place it was just like a garden with extremely luxuriant vegetation. There were garden vegetables, fruit trees, etc. These were finer than could usually be found on most of the farms around Paris.

At first the people of that neighborhood were very much opposed to this sewage water. They said it was contaminating all their wells. At that time, however, no sickness had been traced directly to this water, and at a distance of several hundred yards from this fertilized tract, I went to a spring from which some of the filtered sewage water was flowing. It was perfectly clear—as clear as we have here (when not muddy). I tasted it, and could find no trace of anything disagreeable in it; so that at any rate when this water was returned to the river it was in much better condition than when it went on the ground. I examined the sewage water flowing along these furrows. It was dirty and unpleasant. One could see old garments and hats and all sorts of material, yet the water which came out after the absorption seemed pure, at least to the eye and to the taste.

The engineers were so well satisfied with the trial, although the expense was very great, that it was proposed to fertilize a still larger tract near Paris by the same means. It was a most instructive example of how poisonous materials may be rendered harmless, and also be made to contribute to the sustenance of life.

Mr. Roberts: We have below this city an island called Neville or Seven Mile Island, six miles long, naturally rich on one-half of its length. The other half is high and very sandy. The elevation of that half is about forty-five feet above the river, consequently never reached when the river overflows, while the other half is covered at intervals, probably once every ten years, which makes it a very fertile bottom, and I have often thought the people of that island, if they would utilize the sewage of the city, would make the whole of it a much more fertile garden than it is now. There is an opportunity there to try this experiment. The material could be taken down in scows or boats. That obtained from the night soilers' wagons would be particularly valuable.

MR. WICKERSHAM: I would like to ask Prof. Phillips or Mr. Roberts, in reference to these impurities, if the human system is not so constructed as to reject that which is impure and take only that which is good. I could not help letting my mind run back to the time when I was in business in New Orleans, some forty years ago. There they collected the rainwater in cisterns—upright cisterns about six to twelve feet in diameter, and sixteen feet high. Every house had a system of pipes running from the roofs to the cistern. That water very soon would collect dirt and dust floating in the air, until there would be a scum of probably three or four inches on the surface of the water. It would be covered with vegetation, until after a while the tanks looked like a small garden. The water swarmed with wigglers. They drew this water from underneath, strained and put it on the fire and boiled it, and put it in a pitcher with a lump of ice, and better water I never drank. These insects in the water would not be the same as the bactria spoken of, but it was animal life.

MR. ROBERTS: The more animal life, *ergo* the more oxygen, in the water the better it is. We do not know what the disease germs are exactly; typhus, typhoid and yellow fever each are supposed to have specific forms of germs, possibly, and it is to be hoped all of them are of such a low type that they will not germinate in areated water.

MR. PHILLIPS: There is a German authority who declares sewage water is perfectly safe to drink, and proves it by drinking it himself. This is probably an example of the "survival of the fittest," for the system that is broken by disease would probably be easily affected by such impurities, while one of strong vitality would not be affected.

On motion, the further discussion was postponed, and Society adjourned.

S. M. WICKERSHAM.

Secretary.

THE LICK TELESCOPE.

ENGINEER'S SOCIETY OF WESTERN PENNSYLVANIA,
PITTSBURGH, PA., December 21st, 1886.

Society met at 8 o'clock, P. M. on Thursday, December 21st, 1886. Twenty one members present. The President and Vice President being absent, Mr. M. Lucien Scaife was called to the chair. After the usual routine business was transacted, Mr. J. A. Brashear made the following remarks on the " Lick Telescope."

Mr. BRASHEAR said: This telescope that is now being constructed will certainly reach very near the limit that telescopes are now being made; and, indeed, Alvin Clark himself told me if he had had the advising of the telescope for Mt. Hamilton (the Lick telescope), he would have advised a twenty inch refractor instead of the one that is being built.

The latter is a very fine telescope, yet the question is whether it will be able to do the successful work which is expected of it. When we look back to the days of Galileo and Liepershay and those good fellows, in 1609, when they were making their little telescopes one inch in diameter, and compare them with the telescopes of to-day, it certainly looks like evolution.

And yet when we look at any other science, the science of electricity for instance: When Volta built his little pile what did he expect of it? Nothing like as much as we have to-day. Certainly, nothing in comparison.

When Watt boiled his tea-kettle over the stove did he expect such results as we have to-day? And so it is in any scientific research. We must all bear in mind the beautiful language of the good old Book, which tells us to "Despise not the day of small things."

After the early astronomers and early opticians had done their work, such men as Galileo, Liepershay and Kepler, they found it was impossible to get power out of the telescope on account of the color. If you have a telescope, say of three inches in diameter and a foot long, you have a certain amount of color. Now, if you increase the diameter and do not increase the length, you are still more troubled with the color. But if your glass remains the same in diameter and you make your telescope longer and longer, you can increase the length indefinitely without increasing the color trouble. And so Huygens and Campani and a number of the other early workers kept stretching out their telescopes, until from the seven inch telescope of Galileo they reached three hundred feet. Campani made one three hundred feet long—so long it was impossible to have any sort of a mounting.

However, as we come on down we reach the day of the invention of the reflecting telescope by Newton, I think along in the neighborhood of 1658 to 1660. From that time on, telescopes of that kind began to increase in diameter, and when Short got hold of the work he made some magnificent specimens. I do not like to speak of Short, because he was a man, at least so reported, who was very selfish and wanted no one to know how he did his work. Indeed, this spirit was carried so far that he broke up his tools and every thing that would give a clue to his methods.

We come on then to the days of Herschel, and you will remember that he made telescopes up to forty-eight inches aperture. Lord Rosse made them up to sixty inches aperture.

Delaunay and Hall discovered the method for curing the color, and telescopes were then made up to four and five inches in diameter, and then they had to stop for want of proper glass of large sizes. However, when they had almost given up in despair for this, Guignon, a Swiss weaver, went at it, and finally made glasses up to nine, ten, eleven and twelve inches in diameter. Fraunhofer was associated with him in this work, and they made glasses up to twelve inches aperture made from perfect glass. If I remember rightly, I think it was Fraunhofer who made the fifteen inch glass for the Pultowa Observatory. He was a fine mathematician and a master workman, and his record will last as long as there are opticians and astronomers.

We come along further till the day when the great glass was made for Harvard University. It was made by Merz, of Munich. It is a little over fifteen inches in aperture and was, for the time, one of the finest glasses in the world, and was the mate to the great glass in Pultowa.

Our own Clark began the manufacture of telescopes just about forty years ago. He is now in his eighty-first year. His first glass was made from glass made

in Boston. It turned out very indifferently, though he did some very good work with it. He kept on increasing the size of his glasses, beginning with the seven inch he sent to London, that did such admirable work, until finally he made the eighteen inch glass for the Dearborn University at Chicago. With this glass young Alvin Clark discovered the companion of Sirius (?), for which he received a gold medal from the French Academy.

After that came the twenty-six inch glass at Washington, which was the largest glass ever undertaken up to that time. Cooke had undertaken one of twenty-five inch aperture, and this circumstance called from the great astronomer Lockyer the remark that the "Americans were determined not to be beaten, and so they went one inch better."

From some remarks dropped by Professor Hall, it seems, however, that this glass is not working satisfactorily. At the same time that glass was made, McCormick, he of reaper fame, had one made for the University of Virginia. It was an eighteen inch glass. It has done some admirable work, but it is not mounted to do the work it ought to do.

About the same time the Princeton glass, of twenty-three inches aperture, was constructed, and I may say that this is the finest glass ever made by the Clarks. I have made observations through it, and its limpid purity fairly sets the heart of the optician flowing. The Washington glass has about one hundred bubbles in it, and although these do not affect the glass any, still the Princeton glass has not a single flaw.

Then came the question of the great Lick telescope. A little about Lick first. It is said he was a man of very eccentric character and one of the ugliest and meanest men that ever lived, but that was while living. A man may do queer things during life, and yet something good may exist in him that may not be known until after his death.

Lick was a native of our good old State. So much for Pennsylvania. Born of German parents, in Lebanon county, in 1796, in Fredericksburg. He got into quite a number of businesses before he ever thought of the telescope. He was a piano maker, an organ maker, a peddler, a theatre manager, and I do not know what all. Finally, he drifted down to South America, where he accumulated a fortune of about $40,000. From there he went to San Francisco, invested largely in real estate, and in twenty-four years had accumulated a fortune of over $4,000,000. He enjoyed this for a little while and then turned it all over to a trust. I think Captain Floyd was president of the trust fund. Among his bequests was one of $700,000 for the building of the most powerful telescope in the world. I find that as one of the scientific elements in his bequest that the money should be used "for the building of the most powerful telescope in the world," and he also required that it should be put on a mountain. Finally, at the suggestion of Professor Hilton, some say Captain Frazer, they settled on Mt. Hamilton.

Mt. Hamilton is about fifty miles from San Francisco, and about four thousand two hundred and eighty feet high after the apex has been leveled off. Lick selected this position on the condition that those in charge of the affairs of Santa Clara county would build a mountain road, which they did. The road is something like twenty miles long, a very excellent one, the cost being $75,000. The summit of the mountain was a very small space originally, but the engineers leveled it down and made a very excellent place for the observatory.

This space is about four hundred and fifty feet long by two hundred and twenty-five feet broad. It is said that the fogs from the Pacific never reach the top of the mountain, or at least only little wifts of fog that might be blown up in a storm. Mr. Burnham was on the mountain from the 17th of August to the 16th of October, in 1879, and in those sixty days he found forty-two of the nights were what he called perfect for telescopic work, eleven of them were what we would call excellent, and only seven were bad. We could reverse the order in Pittsburgh, I think, and have seven good nights, eleven fair ones and forty-two bad ones. Continued observations on Mt. Hamilton from that time to this has confirmed the fact that no better point could have been selected for the great telescope.

Now, Leslie's observations in Malta and others from the peak of Teneriffe were such that it was thought good results could not be obtained from mountain peaks, and hence there was no encouragement to put the telescope there until Burnham himself went up and made observations extending through a number of days. It was found that the light of a hélioscope could be seen one hundred and seventy-five miles away, on the coast, and mountain peaks were recognized at a distance of one hundred and thirty miles, so you can readily see the transparency and clearness of the atmosphere. He used a six inch telescope in his work, which would be the same in effect as using on a lower elevation the eighteen inch glass of Chicago, so that will show you the value of the great telescope, especially on mountains where you have such clear skies. What magnificent results may be expected!

The observatory, then, has been pretty nearly finished except the building of the great dome, which is now proceeding. The structure is two hundred and eghty-seven feet long. Nearly every particle of the material had to be brought from below, till finally they found a bed of clay and a fine spring near the summit, to which roads were made.

They have a twelve inch refracting telescope built

by Alvin Clark, a transit instrument, a heliograph, a photographic instrument, and they are all fixed now excepting the great refractor and the building of the great dome.

The dome of the building is covered with coppered nickel, and can be seen quite a distance. The dome for the new telescope is going to be a very difficult task to construct. I believe it has been decided, instead of making the hemispherical dome, to have it a dome of about seven-eighths of the whole sphere. They will rest the dome on a circular wall built of solid masonry, about eighty-five feet in circumference. It will be arranged so that the operators can have their library and reception room for visitors below, where they will not interfere with the making of observations above. They will probably have the chairs for the operator ranged on the end of the telescope, and he will be provided close at hand with electrical appliances that he can move the telescope all around the horizon, so that they will not have to leave the eye piece. The whole thing will be complete in all its details, and the workmanship is all in good hands.

[Mr. Brashear here gave the members a blue print with full description of the mounting of the telescope.]

I will now give you some of the dimensions of this great telescope:

Diameter, seventy-five feet; height over the greatest diameter, that is, the height over the center of the sphere, thirty-five feet; length of tube, fifty-six feet; total length of telescope, fifty-six feet, two inches; diameter of upper end of tube, thirty-nine inches; of lower end, thirty-six inches. The tube will be ventilated.

The diameter of the polar axis is twelve inches upper end, ten inches lower end; the diameter of the axis that moves it north and south is ten inches at one end and nine inches at the other. Length of polar axis is ten feet. The total weight of the plant is thirty tons, and this includes nothing at all about the observatory.

Of course, you can see in the construction of this apparatus, there must be great engineering skill and some of the most accurate work possible. The observatory will be fitted up with all the modern appliances, such as the sidereal clock, etc. The flint glass for the telescope weighed three hundred and seventy-five pounds in the rough. It was cast in Boston. The glass is thirty-six inches in aperture.

The crown glass has a curvature of about eighteen feet. The total weight of the glass complete will be five hundred and eighty-seven pounds. They took great care while working with it, keeping it in a cellar, away from the light, and the temperature kept normal. They have succeeded in making it to satisfy the experts who have been appointed to do the testing.

Mr. Roberts: Did you notice, when you saw the Lick glass, whether there were any bubbles or flaws in it?

Mr. Brashear: I did not see any, although Alvin Clark said there were.

By a Member: If I understand you aright, you say the thirty-six inch glass is about the limit of large refractors. If in the future we are to have any greater magnifying power of the telescope, are we to look to reflectors?

Mr. Brashear: Well, while I make reflectors, I doubt it very much. It was a question whether they would take a seventy-two inch reflector or the thirty-six inch refractor, but the color disturbance in the reflectors decided in favor of the latter.

Prof. Frost: Do you look for any improvement in the quality of glass?

Mr. Brashear: Experiments are now making in that direction and some very fine samples have been obtained, showing an advance in this direction, and it all depends now whether they will stand the physical tests, but I think the softness of the glass I have now in mind will probably be the great difficulty in the way.

Mr. W. L. Scaife then read the following extract from one of the scientific papers on the same subject:

The Lick Telescope Lenses.

Attention was called in a late issue of this journal to the fact that the two great lenses for the Lick telescope, on which the Messrs. Clark, of Cambridge, have been so long engaged, were nearing completion. There has been a plan already adopted for shipping the double lens which is interesting on account of the wonderful precautions it embraces. The two glasses will first be wrapped separately in fifteen or twenty thicknesses of cloth, drawn very tight. The cloth will be cotton, and in order to make it soft and perfectly free from grit it will be washed many times and thoroughly beaten. Next to the cloth will come a thick layer of cotton batting and then a layer of paper. A box made of wood and linen with felt will contain the glasses when so prepared, the felt lining of the box coming next to the paper. The lenses with their covering will be packed tightly in this box. The shape of this box will conform to the shape of the lenses. The felt will be attached with glue, so that no nails will be anywhere near the glass. Outside of this wooden box and enclosing it will be a strong steel box, about the shape of a cube. The wooden box will be tightly packed into the steel box with curled hair. To enclose this steel box will be still another steel box or chest, and the inner steel box will be kept from touching it by a large number of spiral springs covering the whole interior of the outer steel chest. This outer chest will be packed with asbestos, to render it fire-proof, and both of the steel boxes will be made air-tight and water-proof. The outer chest will be suspended by pivots in a strong wooden frame, and a contrivance has been adopted for turning the chest one-quarter around every day during its progress to California. This is to prevent any molecular disarrangement in the glass and avoid the danger of polarization, it being feared that the jarring of the train will disturb

the present arrangement of the molecules unless the position of the glass is daily changed and all lines of disturbance thus broken up. The glass will be insured for its full value—or rather its cost—$51,000, and all the precautions mentioned are taken to prevent any accident to it. It would probably be impossible to replace it, as Feil, who cast it, and the elder Clark, who ground it, are both old men. The glass will be shipped by express.

After which, on motion, the meeting adjourned.

S. M. Wickersham,
Secretary.

SEVENTH ANNUAL MEETING

OF THE

Engineers' Society of Western Pennsylvania.

PITTSBURGH, PA., January 18th, 1887.

Society met at 8 o'clock, P. M., President E. B. Taylor in the chair. Twenty members present.

The Treasurer reported for the year 1886—

Receipts in the General Fund,	$1,593 77
" " Library "	119 66
Total Receipts.......	$1,713 43
Expenditures.	1,287 46
Bal. in Treasury, Jan. 18, 1887,	$ 425 97

The Secretary reports—

Number of members, January, 1886,	307	
" " received, "	25	
		332
Loss by death in 1886............	2	
" by resignation...............	24	26
Number of members now on the Roll,		306
Regular meetings held during year,		10
Average attendance..........	26.4	
Social meetings....................		14
Average attendance...........	9	

3 papers were read; 1 public lecture given.

The Library Committee reports no new works purchased, but quite a large number of serial publications have been bound; a new bookcase purchased, and an *addenda* to the present catalogue is about ready for the printer.

The Committee on Tests, through their Chairman, Thomas Rodd, Esq., reported as follows:

The Committee on Tests of Rail Joints are not prepared to made a final report. A large amount of preparatory work has been done; circulars were prepared and sent to all the principal railway lines in the United States and Canada; these circulars called for considerable detailed information, in regard to rail joints, a subject which was more especially the object of the Society in appointing the committee.

The circular has been very largely replied to, and probably one-third of the mileage in the United States is represented in the replies received. These replies have been tabulated, and the committee hopes to be able to pursue the subject and make a report at some future time.

The subject of rail joints is to-day receiving greater attention than ever before. Very many lines are using new joints, devised by different parties, to overcome the objections found in the older forms. The principal bar in use to-day is the angle bar in different sections and lengths. Several important railroads are now using a bar three feet long. The Fisher joint is being used on many miles of new and old roads, and is, I understand, giving good results.

The Committee feels that justice will hardly be done to the subject at the present day, in making a report, while experience is being gained practically in every direction, as to the merits of the different joints.

Owing to the fact that but very few members of the Committee are able to give any time to the work, and that those few persons are busily engaged, it will not be possible to go into the matter as thoroughly as its importance requires, but we hope to be able to present some information of interest to the Society before very long.

The Committee on the death of Thomas Carnegie made a report, expressing the deep respect felt by the Society for him as a kind-hearted man, energetic manager, and liberal citizen, and their sorrow at the great loss occasioned by his death.

Brief and eulogistic remarks followed the report, which was unanimously adopted.

Mr. E. B. Taylor then delivered his address as retiring President.

Gentlemen of the Engineers' Society of Western Penna.:

Seven years ago, on the 6th of January, 1880, thirty-two members met at the first regular meeting of the Engineers' Society of Western Pennsylvania; by the end of the first year the membership had grown to 184, to-day our Secretary reports a membership of 306. During the first year there was an average

attendance of 51; during the past year, of 26. There were twelve papers read during the first year of the Society's life; during the past year, four—three of these contributed by the same member, and the other by a member who also gave a public lecture for our benefit. So you see, gentlemen, that large membership alone will not make of itself a prosperous society.

The year which has just closed was one of the most prosperous ever known in the history of Pittsburgh, and the prosperity was general throughout the United States, but at the same time it was distinguished by gigantic labor combinations, strikes and riots. The troubles began on New Year's day in New York City, with a strike on the Elevated Railroad; the middle of January saw the coke workers in the Connellsville region to the number of 10,000 men idle; numerous small strikes took place at various points over the country during the months of February, March and April, and on May 1st began the great strike for eight (8) hours as a day's work; this strike was of greatest proportions in Chicago, where at least 60,000 men were out, and was broken by the bomb throwing of the Anarchists. Previous to this were the strikes on the Gould System of Railroads, culminating in East St. Louis in the disgraceful riots which almost equalled the famous riots of 1877 at Pittsburgh, though the loss of property was much less. This strike cost the strikers at least $1,000,000 in loss of earnings, and the cost to the southwestern country was even greater.

These disturbances did not augur well for the prosperity of the year 1886; still in spite of all there was a strong feeling among business men that the country was on the eve of prosperity, and when the labor agitation subsided somewhat and the men went to work in earnest, in every avenue of trade was seen a marked improvement, until the means of transportation became inadequate—though thousands of new cars and hundreds of new locomotives were built, the railroad companies could not handle properly the traffic offered them.

Pittsburgh was wonderfully free from great labor disturbances. The rapid development of natural gas lines gave employment to thousands of men in the manufacture of pipes and laying of them; everybody was busy and earning fair wages.

This general prosperity may have been the cause of the lack of interest in the Society. The members who could and should have written papers for the Society were too busy with their personal affairs to give the few hours work for the benefit of their fellow members. At least "too busy" was the reply often repeated to your President and Secretary by members who early in the year had promised papers.

The reports of your Secretary, Treasurer and Library Committee give you in detail the work of the year, our financial standing, and the increase which has been made in our library. The library is really the most valuable feature of the Society, and many of its members are only just beginning to appreciate the fact.

The Society was very much indebted to one of its most honored members, Mr. J. A. Brashear, for an illustrated lecture on a "Piece of Glass;" this was not only an intellectual treat, but the financial results were very satisfactory. One hundred and nineteen dollars and fifty-eight cents being added to the Library Fund.

In order to, if possible, revive the flagging interest, it was decided to hold weekly social meetings at our library rooms. The first meeting was held March 18th, with twenty-three members present. These meetings were continued from week to week until the summer vacation, and could not be resumed until late in December owing to the disordered condition of the building in which our rooms are located.

The elevator, however, has been completed, and it is hoped that the members will feel enough interest in the Society to give an hour or two in the week to social intercourse with fellow members. The first meetings were very interesting, and gave promise of promoting the Society's welfare.

In our city of Pittsburgh we find, outside of the court house and jail and one or two large buildings, little progress to note.

The streets were kept in horrible condition by the pipe line companies, so that it was almost impossible for the Commissioner of Highways to perform his duties as they should be. The gas companies and railway companies occupying the streets should be compelled to keep them in good order and well cleaned from curb to curb; nothing should be thrown into streets or swept from sidewalks onto them, and any loose material or dirt should be swept up or scraped up and carted away before the streets are washed.

A very little progress was made toward securing a system of drainage and sewerage for the East Liberty valley, but the work should be watched by every one who cares for the health of the city, and not allowed to drag.

The city of Allegheny still continues to draw its water supply from a part of the river contaminated by sewerage of large districts of Pittsburgh and Allegheny, and although we have been assured that the water is not very bad, still enough has been shown to suggest that a city of the second class (as it hopes soon to be) with a population approximating one hundred thousand souls, cannot afford to risk the health of its people in order to save the expenditure of a few dollars.

The exposition building seems to be assured, if its location is still in doubt. The Pittsburgh Carnegie Library is taking definite shape, while the plans for the Allegheny Library have been approved by the liberal donor, and the work of erection will be commenced in a short time.

The new jail has been finished, and the court house will soon be under roof and is nearing completion, and while forming a monument to its distinguished architect, demonstrates the wisdom of the County Commissioners in laying political and local influence aside and giving the work to men of national reputation, who fully perform what they agree to do.

If the progress on the new post office is as rapid in the future as in the past, the twentieth century may see it completed.

The mileage of railroads in the United States was increased by 7,274 miles of main track (not including sidings or second tracks), this increase was the greatest since 1882, and explains in part the great consumption of iron and steel which is reported to have been as follows:

	1886. Gross Tons.	1885. Gross Tons.
Pig Iron,	5,600,000	4,044,526
Steel,	3,700,000	2,612,276

Ore mined in the United States, 10,000,000 tons, and imported, 1,000,000 tons. Iron and steel imported, at least 1,000,000 tons.

While the mileage of railroads has greatly increased, the freight cars are built with a capacity of 60,000 pounds as compared with 20,000 pounds fifteen or twenty years ago. Heavier engines are required to haul the cars, and the bridges and track superstructure must also be increased. So that even if the Collum Inter-State Traffic Bill has the effect of stopping the building of many wild-cat railroads in the West, there will be a legitimate demand for large quantities of iron and steel to replace the light iron bridges and light sections of rail on many of the existing railways.

I am glad to note the great progress that has been made toward uniform freight cars, and the effort made to unite on satisfactory self-couplers and continuous brakes for freight trains, and feel assured that in a few years' time we will see all cars equipped with automatic couplers, which will result in saving of many thousand arms and legs and I may say lives, each year.

Cable railways for our streets, underground cables or conduits, for telegraph and telephone service, electric lights, have made their appearance and should be welcomed.

In a hasty glance on the engineering field outside of the United States, we see little progress on the great Panama Canal, although $107,000,000 is said to have been spent, and $283,500,000 is still required to complete the work, and it is possible the ship railway of Captain Eads may be an assured success before the Panama Canal is finished.

The Severn Tunnel, extending from New Passage to Portskewet, was opened for travel during the year; it is said to be one of the greatest engineering works of the age; passing under an arm of the sea, under water, its entire length 2½ miles, and with approaches being 4½ miles in length, and having been 13½ years in building.

Considerable progress was made on the Frith of Forth Railway Bridge; up to close of the year, 27,232 tons steel, 98,000 cubic yards of rubble masonry and concrete, and 20,000 tons of cement has been used, and the average number men employed on the work last year was 2,545.

The contract for the Hawkesbury Bridge was awarded to an American bridge company, after sharp competition with the best bridge companies of France and England; this must be very gratifying to American bridge engineers, and shows that our designs are as well adapted to other countries as to our own.

To return to our own affairs, I would state that on 20th of April your Society appointed a committee to attend the meeting, at Cleveland, of the Permanent Committee on National Public Works.

The meeting there extended from March 31st to April 2d inclusive. A constitution and by-laws were adopted, and an executive committee appointed to complete plans and call all societies together for action. It is hoped that good results will follow from the work of the committee.

Your Board of Directors met regularly and gave diligent attention to the business of the Society, and while the work of the year from a literary and scientific point of view was not as satisfactory as we could have desired, the financial results were certainly gratifying, showing that your Secretary and Treasurer were faithful to the interests confided to them.

On the 1st of April your Secretary moved his office to the library rooms of the Society, thus enabling the rooms to be kept open for the daily use of members, and many availed themselves of the privilege thus afforded.

In conclusion, gentlemen, I thank you for your kind treatment and the uniform courtesy which prevailed. I congratulate you on having in my successor one who will doubtless fill the specifications laid down by the worthy first President of our Society a year ago as necessary for a president, and which I am free to confess I did not, but I must ask you to do better for him than you did for me; at every meeting of the Society give a good paper, and I hope the year 1887 will be marked as the best in the history of the Society.

The election of officers to serve to January, 1888, resulted, viz.:

President, Alex. Dempster.

First Vice President, to serve two years, J. A. Brashear.

Second Vice President, to serve one year, M. I. Becker.

Directors, E. B. Taylor, A. E. Hunt.

Secretary, S. M. Wickersham.

Treasurer, A. E. Frost.

On motion, a committee of three was appointed to report at our next meeting a plan for opening the library on Sundays. The Chair appointed on this committee W. L. Scaife, T. Rodd and J. H. Harlow.

On motion, a Committee on Programme was formed, whose duty will be to see that a paper is prepared for each regular meeting.

On motion, the thanks of the Society were given to J. D. Weeks, Esq., for his attention to the comfort of the Society during the past year, and also the Iron & Steel Association received the thanks of the Society for the use of their rooms in which to hold the Society meetings.

On motion, the Library Committee was made to consist of four members.

On motion, the Secretary was instructed to procure photographs of our ex-Presidents, to hang in our rooms.

Messrs. G. Follansbee, C. F. Buente and Walter F. Arms were elected members.

At 9:50 the meeting adjourned.

S. M. WICKERSHAM,
Secretary.

ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

Society met on February 15th, 1887, at 8 o'clock, P. M ; present twenty-five members. President Dempster in the chair. After the routine business was transacted, Messrs. R. S. Schultz and Alfred Hurst Read were admitted to membership. The Committee on Sunday Opening made the following report:

GENTLEMEN:—Your Committee appointed to prepare a plan for opening the Library on Sundays, would recommend the following arrangement:

The appointment by the President of a committee of three, with power to open the library room to the members on Sundays, at such times as they may find most suitable; the rooms, on such occasions, being placed in charge of individual members of the Society, who may have volunteered to serve, without remuneration, and who shall be considered competent by the committee.

Although we think it preferable to leave the hours of opening to the Special Committee, yet in case the Society should desire to specify them, we would suggest the hours from 2 to 6 in the afternoon, and 7.30 to 10 in the evening.

Respectfully submitted,

W. L. SCAIFE,
J. H. HARLOW,
THOS. RODD.

A motion was made and seconded to lay the report on the table.

MR. HARLOW said: At the last meeting, when Mr. Scaife offered his resolution, I was rather opposed to the suggestion, and afterwards, when meeting our Chairman I expressed something of the same idea. But I remember when I first came to Pittsburgh I was here alone for several months, and I found Sunday a very long day. I often wished I had some place to go to spend it.

Within the last year I had occasion to spend several months in a strange place, where I found the library was open, and when I compared the two cases, I concluded it was a good thing to have the library open. I make this as explanation of my change of position since the last meeting.

MR. DANSE: So far as I am personally concerned, I do not think there is any probability of my ever using the library rooms on Sunday, for the simple reason I have a home to spend it at, and other ways of occupying my time. But as the gentleman who has just spoken has said, there are a great many persons who have no place to go on Sunday. I have no doubt there are a great many of our members in that position, and they would go to a quiet place. It could not possibly have a bad influence, and I think would certainly have a good influence. I think it would be beneficial to them, and that a number of them at any rate would go, and would be very glad to get a place where they could go, and not have to lie around a boarding-house all day. I am opposed to the motion (to lay on the table), and regret its having been made. I hope it will not be carried.

MR. DEMPSTER: I think that perhaps the motion may seem uncourteous, but I do not suppose it was intended to be that, but I think you ought to consider well before you adopt this. Now I always go to the bottom of a thing. I look to see what its practical workings are, and the reason for adopting any certain course. The last gentleman who has just spoken has dealt in generalities, while the first gentleman mentioned his own individual case, but he did not say whether or not he could not have entertained himself at any other place. It occurs to me in this way: Shall the Engineers' Society of Western Pennsylvania put itself in the shape of violating the law, of violating the opinion of the community, the expressed opinion of the community? Will it do any good? Are there any gentlemen here who have no place to go on the Sabbath day? Are there any gentlemen here who need the information so much as to require the opening of the rooms?

These are the questions that present themselves. If the library can be used on week days, for the benefit of every member, or of each member, then there can be no excuse for opening it on the Sabbath day, and even if this is done the danger is that it will degenerate. I have the greatest confidence in the gentlemen who offered this resolution. I know their good intentions. I know it is their desire to benefit the Society to the utmost of their ability, to the utmost extent it can be done. I know that and I feel it, but I think they are mistaken. I think they are mistaken in their action, and I think they are mistaken as to what the results will be.

I may be mistaken myself, and those that think with me, but I still think we ought not to open the library on Sunday until all other means are exhausted whereby members can get all the benefit they want to derive from the library in other ways.

If you noticed the report of the Secretary at the last meeting, it was, that on certain Friday nights not a single solitary individual came. Now if there are persons who are thirsting for knowledge and want to slake their thirst at this fountain, why not come and drink on Friday night, when they have an opportunity, or at any other time when they have an opportunity?

Then we are placing ourselves in the position of opposition to public opinion, in opening our rooms on the Sabbath day. Now I do not want to place myself in a position of trying to think for anybody else, or to set myself up as the conscience of any man. It is every man's right to think as he pleases and to do as he pleases, so far as a gentleman is concerned. I do not want to place myself in that position, but it is a different thing when we are members of a society. We are an organized body, and that organized body places itself in the position of opening its doors on the Sabbath day, its front doors, while the saloon opens its back door. I think it will be a bad proceeding, and it will be an entering wedge to the dissolution of the Society.

This is my candid opinion, and I hope it will not prevail to open the rooms on the Sabbath day. I think that there are other means which can be adopted, whereby every member of this Society who desires to get the benefit out of the books that may be derived, can do it on another day from the Sabbath day.

Mr. KIRK: I would like to say a word on this subject. I seconded the motion to lay on the table, mainly from the reason, that I read many years ago a work by Sir Matthew Hale in which he showed by the figures, actual statistics, that the lawyers of London were more given to insanity than any other class of men in the kingdom. He attributed it entirely, without any hesitation at all, to the continual drag of the mind in one channel. That the lawyers, as a class, paid no attention to the Sabbath as a day of worship. They were simply drag, drag, drag all the time. They were violating the laws of nature that calls for a cessation of trains of thought as much as it calls for the cessation of the train of labor on that day. And I have watched that through my life in a great many instances. I say that the engineer that cannot satisfy himself with learning in his profession in the six days of the week, will not gain anything by work on the seventh. I have never yet seen a man prosper by working on the Sabbath day.

And as to the necessity of the rooms being open, I think my lot has been cast at times among as many lonely places on the Sabbath day as the average business man. I have repeatedly been caught away off in the lonely villages of the Allegheny mountains, where it seemed to me that even civilization was very scarce, and yet there a man can find good company, and services appropriate to the day, in keeping with the march of intellect on that subject, and I do not see, laying aside, even ignoring for the time any religious application to Pennsylvania, the law of the Sabbath is the law of God, and I say that it is not necessary and will not be for the good of the Society to open the rooms. It never has been, in my observation and knowledge, of good to any society to do such a thing. There is no necessity for anyone being lonely in Pittsburgh. When I came to Pittsburgh I was isolated and alone; without a single acquaintance. I never had occasion to go to a library room. If men are so disposed there are plenty of Sabbath Schools and preaching, and that is a change from the channel of thought That would not be found if we kept open our books and studies on the same subject.

I say it is unnecessary; it will be an injury to the Society to do it, and a disgrace to the members, as has been said.

Mr. BROWN: I think that Mr. Kirk's speech is the greatest thing in favor of opening the library. It is a fine thing for members to talk this way who have books in their own libraries, but now there are others that do not possess these advantages who may wish to consult particular authors. Every engineer cannot afford to carry a universal library around, and it would be a very good thing for many of them to consult authors that they cannot afford to buy for themselves.

Mr. ROOD: Mr. Chairman, it seems to me that this is an organization or club composed of intelligent men, all able to judge for themselves. There may be some good reasons on both sides, but I agree with the views expressed by the gentleman, that if there are members of the Club who desire to use the rooms of the Society, they ought to have the right and be perfectly free to do it. They do it for no bad purpose. They will not all go to Sabbath Schools and churches. If it turn out that the privilege of going to the rooms be misused, it would be a simple matter to stop it. I think it would be a good thing to give it a trial.

Mr. HUNT: The only preface I will make is, to state first, that I don't want to be considered as a hypocrite. I am sorry to say I do not attend Sunday School as often as I ought. Further, that I read books akin to those we can find in our library, on Sunday, and I read them more than I do those that brother Kirk has referred to. And I again want to say, that I feel we ought to have the right, as members of the Society, to the use of the library more than we have had; and I will go still further, that we ought to have the

right to use it if we wish to on Sunday. That far I do go. On the other hand, I think we have arguments put from the other side in a way that surely we ought to consider seriously before we decide to the contrary.

This is an organization, and while we as individuals could each of us take a course which nobody would have a right to question, as an organization such a course will be questioned and will be criticised. I should be loath to be held up in this matter as being one belonging to an organization that were among the first to break down the Sunday laws of Pittsburgh.

I was not used to that when I came to Pittsburgh, and when anything got wrong with the furnaces I had been in the habit of taking Sunday to make repairs. I had always done so until I came to Pittsburgh. But when I went to do that on my first Sunday here, Mr. Parke told me that was not the Pittsburgh notion; was not the Pittsburgh way of doing things. I had to quit it. And now I am very glad to say that I honor the Sunday observance practiced here. I have come to respect the strict Puritanical notion of Sunday in vogue here, if such a term may be used; although, as I have said before, I do not always live up to it. And I am going to vote to table this resolution, because I believe, as an organization we should not keep our library open.

I would like to hear from the President before this matter is put to a vote, as to the other way that he had in mind whereby our library could become of more service to the members. I hope he has something that will give us new light on the subject. If he has a way whereby the books could be made of more practical use to us as books of reference, I am in favor of that; but as an organization, as it would be injurious to us, we should not keep the rooms open.

Mr. Frost: There is one point which I have not heard mentioned to-night, and while I do not take a personal view of the matter, it may be as well for the Society to consider it. We have a membership of nearly three hundred, with an attendance of less than fifty here to-night. There must certainly be some objection to the minority taking so important an action as this. But that is not just the point. What is the Society going to do for its support? Is the minority going to support it by its fees, or is it going to provide other than the present way of maintaining it, whereby our expenditures will be met as they have been in years past, so that we will come out with a balance each year. This is a prudential consideration. I believe there are many who might be styled contributing members, who would be unwilling to take an action which they do not consider as necessary. I have in mind a member who has contributed largely toward the building up of the library, whom I feel certain would not be in favor of this step. I do not know what may be the views of many who are absent to-night, but we ought to take that into consideration also. I have personally no objection to the opening of the rooms so far as the use is concerned for legitimate purposes, but here is the prudential consideration that we ought to acknowledge to some extent.

Mr. Scaife: I had not intended to say anything, having with the other members of the committee formulated a means by which the library could be opened if desired. I thought the committee had done their part in bringing up the motion. Originally it was done because I had heard a number of members express the wish to get into the library on Sunday, and I thought they had a right to do so if they desired it. I considered it the only proper way that they should have access to the library would be by being authorized by the Society. I take a rather different view of the moral aspect from our worthy President. This is the way it appears to me: I consider this library a co-operative library, belonging partly and wholly to the members of the Society. If they wish to make use of it, I consider they have a right to do so, in so far as they do not contravene anything that is right. Therefore the question comes up, and did come up to me, whether it is not right for the library to be open on Sunday. I have thought seriously of the matter, and with a number of gentlemen who have spoken, I consider it not only right but beneficial to the Society. If the members wish to make use of the library, certainly they have the right to make use of it, and if they don't wish to make use of it there is no use in opening it. This library being a co-operative library, I consider it no worse to make use of it than we to make use of our own libraries on Sunday. I do not believe there is a gentlemen here that does hesitate to go into his library and read any book that is there, taking it for granted that there is no improper books in their libraries. I do not know whether the other members of the Society will take this view of the matter or not, but it seems to me the proper one. And there is no reason why members who have libraries of their own should make rules for others who have not.

Mr. Monroe: I certainly do not intend any disrespect to the very worthy committee that has presented this report, but I take it from a moral point of view entirely. I could not have voted for the report, and so I made the motion to lay it on the table, believing it better there than to bring it up this evening or any time. And I think to keep the library closed will increase our finances more than if we open it. I have understood from different ones it will have that effect. I certainly shall vote against opening it.

Mr. Miller: Mr. Chairman, I have been very highly entertained by the different gentlemen who have made remarks here to-night. I have been a member for fifty years of mechanical institutes and

such like, and I have never belonged to one that was open on the Sabbath day. I am not that very religious that I would want to dictate to other people what they should do, but I think if the Engineers' Society of Western Pennsylvania would open their library, it would be a stepping-stone to the theatre, to the race course and to the opening of saloons in general. If engineers, who are considered to be all pretty well informed gentlemen, think it is right to open on the Sabbath day, why should not these other people who want to go to the theatre, to the concert or the coliseum, or such things? I, for one, will vote against it. I have been a little particular, and I have found that the first stepping-stone soon leads to others. I have been in France, where they work until twelve o'clock on the Sabbath day. I have seen them pave the streets in Geneva until twelve o'clock on the Sabbath day, and I think no good was derived from that; and I further think, that unless there is a more urgent necessity than at present exists, that it will be a bad example for the engineers to set before a community like Pittsburgh. I will vote against it.

Mr. Scaife: I would merely like to remark, that when in the cities of Europe, that so far as my observation went, generally the saloons, balls and so on, were open first and the libraries were the very last things that were opened. I have an idea that if the libraries had been open sooner, and the people had been educated to use them, the saloons would have been a great deal longer being opened.

Mr. Dempster: It is all very well to make assertions and speak for other people who do not speak for themselves. It is a plain fact in my opinion that you are violating the civil law of the city. You are throwing a flagrant offense into the faces of the church-going people of the cities of Pittsburgh and Allegheny. Now, can we afford to do this? Can we afford to violate our charter? We are now going to take action that will be a violation of the law of the city, in my opinion. But it is for the majority to decide. But, gentlemen, I will say this, in all kindness and all candor, let each gentleman speak his own personal opinion.

So far as Mr. Scaife's point is concerned in the main, I agree with the correctness of it in one point of view, that we are a co-operative association, co-operative as to the right to use the library, but in order to adopt your views we shall have to change our charter. And, finally, that a man may do as an individual what as a member of a society he cannot do.

The question was then put, and by a standing vote of 12 yeas, 10 nays, the report was laid on the table.

A paper on "High and Low Water Lines in the Allegheny, Monongahela and Ohio rivers, from Sewickley to Freeport and McKeesport," prepared by T. P. Roberts, was read by Geo. W. Brown, Esq., and discussion deferred.

Mr. Todd read the "Specifications for Bridge Building by the Keystone Bridge Company," after which the meeting adjourned.

S. M. Wickersham,
Secretary.

ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

At the regular monthly meeting, held on March 15th, 1887, twenty-seven members and eight visitors were present. President Dempster in the chair. After the regular business was transacted, Messrs. Wm. G. Wilkins and Chas. S. Pease were admitted to membership. Mr. Metcalf offered the following preamble and resolution:

WHEREAS, We have learned with deep regret of the death, in the prime of life, of the eminent engineer and public benefactor, James B. Eads.

Resolved, That we do hereby testify our appreciation of his great character, and offer to his family our earnest sympathy in a loss which comes to us as a national bereavement, and to them as a calamity too deep for expression.

On his motion of sympathy at the death of Captain Eads, Mr. METCALF said: It is fitting that we, as a Society, in this city where Captain Eads was known for so many years, and where he had so many friends, should express our sense of the loss occasioned by his death, and that we should send a copy of the resolution to his son, and also have it on our minutes.

Mr. A. DEMPSTER: Ere we pass this resolution, and pay our tribute of respect to the memory of him whom we knew but to admire for his genius, and esteem for the sterling qualities of noble manhood, deeply stamped upon his character and conduct, permit me to express a "word of sorrow" at the loss sustained, and of sympathy to those who mourn his death. My mind recurs to the meeting he addressed here about one year ago. I see him as he stood where I now stand, explaining with the convincing eloquence which was the offspring of his abounding confidence in the feasibility of his project, the details of the great "Ship Railroad," which as a band of steel across the chord of the continents would connect the Atlantic and Pacific oceans, and safely transfer the leviathans of commerce from the one ocean to the other on their mission from China to Europe.

Fully impressed with the correctness of his ideas, wrought out in detail by his masterly mind, he saw the end from the beginning, and with the pennant of success ever present to his eyes, he went forward under the impulse of that complete confidence in his own ability which accomplished the construction of the St. Louis bridge, to span the mighty river and to direct its onward flow into the gulf, and make the Father of Waters pay tribute to the commerce of the world. The scope of his genius and grasp of his mind were far in excess of the most of men, and he occupied a commanding eminence in the profession on which his genius reflected so much credit; but I acknowledge my inability to do full justice to his ability, and will content myself in laying a simple flower of friendship on his bier.

The preamble and resolution were adopted.

Mr. E. THACHER then read:

SPECIFICATIONS FOR RAILWAY BRIDGES.

When the accompanying general specifications of the Keystone Bridge Company were prepared, the writer had no idea of making them the subject of a paper before this Society; but by the solicitation of some of the members they are brought forward, in the hope that they may bring out some discussion that may be of interest and value.

After giving requirements as to form of truss, stress sheet and clearance, comes a description of roadway for single and double track truss bridges and plate girders. In single track bridges the track stringers are placed nine feet between centres, more than the usual distance. This was done for several reasons: first, it is more economical than a lesser distance; second, any lurch of a train in passing over the bridge occasions less irregularity of stress and deflection in the two lines of stringers, and the track is maintained more nearly level than by a lesser distance; third, this distance allows hook bolts fastening the cross-ties to the outer flange of stringers to pass through the guard rail, the latter being at a proper distance from the rail; fourth, the track stringers being in close proximity to the bottom chord assist the latter in resisting reversed stresses due to wind. The writer endeavored to prove in his discussion of Joseph M. Wilson's paper on "Specifications for Strength of Iron Bridges," Transactions American Society of Civil Engineers, June, 1886, that without some such precaution, the wind usually specified would collapse the great majority of bridges in this country.

In double track bridges the conditions are very different, and the track stringers are placed 6′ 6″ between centers for each track. This is as economical as any other distance; it gives about equal spacing

of stringers, and no trouble in attachments to floor beams. It allows hook bolts fastening cross-ties to the inner flange of stringers to pass through the guard rails. It allows of the use of smaller cross-ties than a greater distance, and as the weight and width of bridge are much greater than in single track bridges, there is little or no danger of collapse.

The specifications consider ten different classes of rolling loads, each composed of two engines followed by a uniform train load. The engines it will be seen from the diagrams, range from a 44 ton narrow gauge consolidation, to a 112 ton decapod, and the following loads from 1600 to 3000 pounds per lineal foot of bridge. Such loads have been selected as we are most frequently invited to base estimates upon, but they embrace but a limited portion of the number submitted to us.

A table has been prepared giving the equivalent uniform lôads corresponding to these ten different classes of wheel loads for spans ranging from 10 to 300 feet in length. For spans up to 70 feet the loads are calculated from a maximum center moment, and for spans of 75 feet and upwards, from a maximum end shear. These uniform loads give essentially the same results as the wheel loads, and are a great convenience in calculation; for example, take a 170 foot span containing ten panels of 17 feet each, rolling load class 7; opposite 17 (one panel length) find 6180 pounds, the load per lineal foot for which track stringers must be calculated; opposite 34 (two panel lengths) find 4610 pounds, the load per lineal foot of bridge for which floor beams and suspenders must be calculated, and opposite 170 find 3350 pounds, the load per lineal foot of bridge for which truss members must be calculated. The chord stresses for spans exceeding 200 feet in length are, by specifications, calculated from the uniform train load, and an excess load of about the length of two engines, the equivalent loads in the table giving for such spans too great a result in panels towards the center; but if the engines are preceded as well as followed by the train loads, the equivalent loads given in the table can be used for the calculation of the chord stresses in spans of any length, and the results will be essentially the same as from the wheel loads placed in position for maximum effect. The great variety of specifications adopted by the different railroad companies, not only as regards loads, but unit stresses, impacts, and other conditions, add greatly to the cost of estimating by bridge companies. With many hundreds of estimates on record, and in condition for convenient reference, it rarely happens that any old estimates can be found to suit a case in hand.

It would be very desirable if the representative bridge and railroad engineers in this country could get together, and agree upon a bridge specifications that could be truly called American Standard.

Very few railroad companies use such engines as their bridges are calculated for, and four or five varieties of rolling loads should be sufficient to satisfy the wants of all. I do not know as it would be possible to agree upon the quality of material, as some attempts have been made in that direction, but a great step in advance would be made if all other requirements were made uniform.

We will next consider the unit stresses given in paragraph 13.

The formula for wrought iron tension members is a modification of Launhardt's, proposed by Prof. Wm. Cain, and the same as used by the Penna. R. R. I am satisfied with this formula. To my mind it meets the question of impact perfectly, and is a vast improvement on the arbitrary rule of thumb or percentage plan given in most specifications; for example, to use a working stress of 7000 pounds per square inch in the flange of a girder 20 feet long, and 8000 pounds per square inch or 14 per cent more if 20′ 1″ long, is not consistent, and shows either a waste of material in one case or a deficiency in the other, whereas by the formula the working stresses give an unbroken line, and the change is gradual from one condition of loading to another. Launhardt's formula originally proposed for working stresses, and obtained by substituting the constants determined by Wohler's experiments, using a factor of safety of three is

$10000\left(1 + \frac{\text{Min. stress.}}{\text{2 Max. stress.}}\right)$. This allows for an unlimited number of repetitions of stress, but not for impacts.

The following table compares the results obtained by this formula with those obtained from Prof. Cain's, and it will be seen that the allowance for impact by the latter ranges from 0 for all dead load to $33\frac{1}{3}$ per cent. for all live load.

	Min. / Max. Average.	Working Stress.		Ratio.	Allowance for impact by Cain.
		Launhardt.	Cain.		Per Ct.
Counters	0.0	10000	7500	1.333	33.3
Track Stringers	0.08	10400	8100	1.284	28.4
Floor beams & Suspenders	0.15	10750	8625	1.246	24.3
Bottom Chords, 100′ span	0.25	11250	9375	1.200	20.0
Bottom Chords, 200′ span	0.333	11666	10000	1.166	16.6
Constant Loads	1.000	15000	15000	1.000	00.0

The values of $\frac{\text{min.}}{\text{max.}}$ given in the table are average cases. Taking the stress allowed on the bottom chord of the 200′ span, viz., 10000 pounds as a basis, we will find the allowance for impact on live load as follows: Counters, $33\frac{1}{3}$ per cent.; Track stringers, 25 per cent.; floor beams and suspenders, 19 per cent., and bottom chords of 100′ span, 7 per cent. Results by this formula are believed to be well within the limits of safety and do not exceed average present practice.

The allowed stresses on steel tension members are 1.4 times greater than on iron, this being about the ratio of ultimate strength.

The formulæ for iron columns with flat and pin ends result from the experiments made by G. Bonscaren, Clark & Reeves, James Christie, Joseph M. Wilson, and the United States Government at Watertown, as platted by Mr. Wilson and shown on Plates XXXV and XXXVI of the paper heretofore mentioned. These plates give the ultimate strength of the columns tested in terms of $\frac{l}{r}$. Lines were drawn connecting the maximum and also the minimum values of breaking strength. The formulæ are the equations of the lines passing through the centers of gravity of the inclosed areas, allowing a factor of safety of five, and represent the average strength of the columns for all values of $\frac{l}{r}$. The formula for columns flat at one end and pin at the other was taken as a mean between that flat at both ends and that pin at both ends.

The formula for steel columns with flat ends is founded on the careful experiments made by James Christie, as recorded in Transactions of the American Society of Civil Engineers, August, 1884. The formulæ for flat ended iron and steel columns give the same strength for a value of $\frac{l}{r} = 200$, which agrees well with Mr. Christie's experiments. It is presumed that this relation between iron and steel will hold good for pin ended columns, but in the absence of experiments on steel it was deemed advisable to err on the side of safety, and assume that the strength of iron and steel pin ended columns was the same for a value of $\frac{l}{r} = 150$, and the formula thus considers. As was the case in iron, the formula for columns flat at one end and pin at the other is a mean between that flat at both ends and that pin at both ends. It will be observed that the formulæ for compression members do not consider Launhardt's formula, the latter being used only when the allowed stress by it is less than by the compression formulæ, which is rarely the case. Launhardt's formula as modified provides for the fatigue of the material, and for impacts, and the compression formulæ provide against failure, partially or wholly, by flexure. Columns deflect very little until near the breaking point, and repetitions of stress within safe or working limits are not repetitions of flexure. So long as the column is not subjected to an eccentric stress due to flexure, and so long as the stress does not exceed the limits of Launhardt's formula it cannot be injured by an unlimited number of repetitions of such stress.

The formulæ for the compressed flanges of beams and girders recognize the well known fact that when the flange is unsupported for any considerable distance, failure happens by bending sideways; they are the same as given by Prof. Rankine, except the numerator has been changed to suit different conditions and materials. For wrought iron, the allowed stress for rolled beams is about twelve and one-half per cent. greater than for riveted, some experiments seeming to give about this difference in their favor. For steel, the allowed stress is the same for riveted as for solid rolled beams. The reason for this may be found under the quality of material, steel being used having an average ultimate strength of 72,000 lbs. in one case, and 64,000 lbs. in the other.

The formulæ for lateral struts allow a working stress about twenty-five per cent. greater than for pin ended columns in truss members, a larger apparent factor of safety than for lateral rods. This is due to the fact that such struts are frequently shallow, and much weakened by the deflection due to their own weight.

The formulæ for members subject to alternate tension, and compression consider what I believe to be a fact, that so long as the member satisfies the compression formulæ, and is within the limits of Weyrauch's formula for alternate stress, it is safe under all conditions.

The rules governing combined stresses, when the cross-ties rest directly on the top or bottom chords, are believed to be safe. I, however, consider such construction inferior at best, but some will have it, and sometimes it is difficult to avoid. The stresses are really indeterminate, and much affected by deflections at panel points, character of splices, and position of pins. The stresses allowed on pins and rivets do not differ materially from ordinary present practice, and are believed to be abundantly safe. The formulæ for timber columns are derived from Hodgkinson's experiments on columns of Dantzic oak, red deal and French oak combined with the experiments of Capt. Rodman, U. S. Engineer, on the transverse strength of American timber. It was found that the column strength by Hodgkinson's formulæ for the different timbers experimented upon bore nearly a constant ratio to their coefficients of transverse strength. These timbers were very dissimilar in character, some being leaf woods and others pine woods. It was therefore fair to presume that the same relation between column strength and transverse strength would hold approximately true for all kinds of timber, and the formulæ thus consider. The factors of safety have been influenced to some extent by practical considerations, and are about eight for yellow pine and white oak, and seven for the other varieties.

The allowance for wind pressure amounts to about thirty pounds per square foot on the exposed surface of both trusses, and on a train surface ten feet high.

The allowed stress on lateral rods is higher than usual, being 20,000 lbs. per square inch for iron, and

28,000 lbs. per square inch for steel. The subject of wind stress has been discussed time and again, but the fact remains, that a wind exerting a pressure of thirty pounds per square foot will collapse the great majority of bridges standing in this country to-day, let the strength of the lateral rods be what they may; and as these bridges continue to stand year after year, I am forced to the conclusion that they are not exposed to anything like this force; but allowing that they are exposed to a force equal to, or greater than thirty pounds per square foot, there will in general be compression even in the center of the bottom chord before the stress on the lateral rods passes the elastic limit, and any additional section would be thrown away.

The specification does not consider initial stress except when in excess of wind stress, for the reason that these stresses are opposed to each other, the initial stress going out as fast as the wind stress comes in; neither does it provide for additional strength of the lateral systems on account of centrifugal force. As it is not probable that a train will be running at a high speed when exposed to a maximum wind, and as the wind specified is of itself sufficient to upset an ordinary passenger car, such provision does not appear to me necessary.

For plate girders one-sixth of the web is counted as flange area in each flange. Specifications of late quite frequently omit this, and one even goes so far as to omit one leg of the angles. So long as correct methods of calculation are known, and can be applied without difficulty, I can see no occasion for resorting to incorrect and wasteful methods.

The provision that should be made to guard against flexure in the webs of plate girders has not been very well determined, and the practice of different engineers differs quite widely in this respect.

The formula given on page 22 is unquestionably safe and results usually in providing stiffeners from 2′ 9″ to 3′ 6″ between centers at the ends of girders, this distance increasing gradually towards the center.

The formula for camber provides that the chords shall be straight and square bearing when subjected to a maximum stress. It is the general formula for the deflection of beams supported at the ends, and uniformly loaded modified to suit open web girders. The values of the constant are obtained by applying Mohr's formula to a variety of cases. Mohr's formula gives reliable results, but requires more time than one is likely to give to it. The formula of the specification is readily worked, and gives essentially the same results.

The provisions for workmanship are such as will secure first-class work.

Engineers, perhaps, never will agree regarding the quality of material suitable for bridge work, and I do not care to dwell upon this question.

The specification requirements are believed to be as high as can be observed with any degree of certainty, and higher than many manufacturers will accept. My own impression is, that the use of the highest obtainable grades of wrought iron is not a matter of grave importance so long as uniformity is maintained, and I think the requirements might quite likely be lowered to advantage in some cases.

Mr. Metcalf said: I would not like to say I did not understand what was said, but I think the gentleman has taken advantage of us, and I don't think many of them knew what he said unless they have better ears than I have.

I will say one thing in regard to the general specifications: I notice in reading it over some days ago that the specifications require all bends of steel should be made cold. It is but a short time since I read a paper before another Society, in which I stated that my opinion was that no cold bends should be allowed in steel.

The gentleman also speaks of steel rivets. Being a steel man I wish to express my utter want of faith in steel rivets. I do not think they are reliable or ever will be. I have been engaged in the last three or four weeks in taking out something over one hundred steel rivets from our boilers. We have been watching the matter closely, but the inspector went into the boiler last week, and informed me there were forty-five rivet heads in that boiler that were broken off, and we proceeded to have them replaced with good iron rivets.

I can give a reason for that, I think. The trouble is not with the material, but it is in the handling. You cannot expect a rivet boy to heat every rivet he gets hold of, without the knowledge of the physics of the material that he ought to have, with the care it requires, and he will heat it just as he would an iron rivet, and that heat will make any steel very coarse grained, crystalline and weak. When the rivet is driven of course the head being struck is refined by the hammering, but the body of the rivet gets none of that work, and the result is, the heads of the rivets drop off.

I have just made a contract to-day for a pair of new boilers, and I have particularly specified the very best steel for the shell, and the very best iron we could get for the rivets. Certainly, unless I saw some good reason to change, I would never allow a steel rivet to go into any structure in which I had any interest.

Mr. Davison: I am one of the fortunate ones who heard Mr. Thacher very well. I would like to ask a question, not to discuss at all, and that is, if the table of equivalent uniform loads given for calculating the stresses in bridges is derived from the wheel loads of the two coupled engines and the following loads specified.

Mr. THACHER: Yes, sir. The engine was placed in every case.

Mr. LINDENTHAL: Mr. Metcalf's remarks on rivet steel reminds me of an experience with it we had. We used steel rivets in some boiler work, and some of them had to be taken out—had to be cut out—and proved to be much tougher, and much harder to get out than iron rivets that the men had handled before or I had seen before. But there were some other places where the rivets came off very easily, merely from striking them. The difference seemed to be this, that the heads made by hammer during the riveting operation were good, while the heads that were made manufactured or by the manufacturer of rivets came off with a single blow. So it seems to be due to the manufacture of the rivet heads; with the process by which steel rivets are made now, they do not make a good rivet.

Mr. METCALF: Mr. Lindenthal's statement agrees precisely with what I stated. The rivet heads that came off in those boilers were made when the rivet was formed in the machine. I do not remember any case where the outside head came off that was formed by hammering, so his statement agrees precisely with what I said, that where the rivet received a hammering by the riveters the steel was good, and the head was good, but when the rivet boy had heated the whole rivet, at a very high heat, such as used in iron, above a lemon heat, that will make any steel very coarsely crystalline. That is hard to question, it is so well known to us from our every-day life, as we all come in contact with it.

I have had my men come to me making complaints about steel, and they tell me it was made by so-and-so. I would say, "All right, I will go and see him;" and we do go down and see him, and that is all there is of it. This trouble is recorded in the steel always. There never can be any question about that at all; and I say that when a rivet is heated to that degree this head which has been formed in the machine, and well formed, and the work of the machine would make the head, is made good by the hammering, but the shank of the rivet has been put into that very coarse crystalline condition, and all the hammering of this rivet does not put the necessary work on that to restore it to the proper condition to stand the strain, and it is left very weak.

Mr. Lindenthal's experience accords exactly with mine, and I think the explanation I gave of it was true. Iron being fibrous, and being a laminated material, is not injured by a high heat in that way, and the fibres running down from the shank of the rivet hold it there, and I believe it is a better and safer rivet, and always will be, than steel.

If we could be sure that no rivet would be heated any different than an orange color, the steel rivet is stronger than an iron rivet, as the steel in itself is stronger than ordinary iron, but unless you can be sure of that, every rivet that is overheated will certainly be weak, and the finished head is very liable to drop off.

I do not think I exaggerate when I say we have taken out of our boilers in the last three weeks 150 rivets. A good many I will say were eaten by acid, we having gotten a dose in our boilers, and some of the rivet heads may have been eaten off, but still for reasons I have given many have dropped off independent of this cause. The trouble was with the machine made head, and it was just because the rivet had been heated in the shank when put in.

Mr. WHITE: Do I understand you, that the work of the machine heading the rivet leaves the steel in a different condition?

Mr. METCALF: I think it would, sir. I have tested a good many rivets made in this way, and found them unsatisfactory.

Mr. KOCH: I am a stranger here, but may introduce myself as the manager at Spang's, and will say something in connection with boilers, if I have your permission.

I have been engaged in making steel for nearly twenty years. I worked in the first open hearth furnace that was ever built. We have boilers running in England now, in which the steel is over eighteen years old, in which the rivets are entirely of steel, with steel plates, and those boilers have never had anything in the way of repairs.

Then I remark this. The specification for our boiler steel in the old country were like this: phosphorus is not to exceed .05. We found that it would not do to exceed this, and that when we did we could not sell any steel, and the trouble here is, the trouble right along with the steel that is being made here today is, that you allow your phosphorus to run to .07 and .08. The moment you get over .05 you are running into dangerous steel. That is to say speaking of boilers.

I made rivets sixteen years ago, and since I began making the rivets and boiler plates largely by the open hearth process, our specifications have been about as follows: that the carbon in no case was to exceed .15, that the phosphorus was not to exceed .05, sulphur .05, and manganese not to exceed at the outside one-half of one per cent. And we were bound down to this chemical analysis; and were bound down very strictly, and we had no trouble then where these specifications were followed strictly.

If we allowed the phosphorus to exceed .05 then we had trouble, and if you run up to .08 you are getting a very dangerous condition of things.

Then there is another point. There are two kinds of steel, one kind made up of pig iron, and then the

mixture made with scrap. This is what we call the scrap process, and is entirely barred for the manufacture of the best boiler plate.

If you will look into the matter of the process employed by the leading manufacturers in the old country you will find the Siemens process is put down by them as the one for use. I found them making steel at Spang's by the scrap process, using 60 to 80 per cent. of scrap. I put a stop to that at once, and I started on the regular process. I simply took about half scrap and half pig, and now we are making a very fine material, and taking less scrap and more pig, using the best Spanish and Mokta ore.

While I believe that the steel made by the Siemens air process is better than with the scrap process, yet you can produce a very fine material with the latter so long as you keep the phosphorus below .05.

Mr. Dempster: Will Mr. Thacher let us hear from him on the steel rivet question?

Mr. Thacher: Well, so far as steel rivets are concerned, we have used them extensively in bridge work, and of the many thousands driven and carefully inspected not a solitary head has dropped off, and to knock them off with a sledge is a very laborious undertaking. So far as our experience goes, steel rivets are satisfactory, and should be used in steel bridge work; I cannot speak as to boilers. The cold bending of steel mentioned in the specification applies to nothing more important than thin cover plates, for such, cold bending being considered preferable to working hot without annealing.

Mr. Metcalf: I hope we will have such a stranger drop in on us every evening. We enjoyed his remarks very much, and I agree with every word he said. I repeat what I said in the beginning, that the steel rivet, properly heated, is just as much stronger than the iron rivet as the steel is stronger than iron. I still believe that the rivets were faulty because they were overheated, and I think the gentleman will agree with me that any steel that is overheated until the grain is raised, and he knows what that means, and is not subsequently worked, is left coarse grained and weak, and the shank of the rivet is heated into that condition, as I believe it is, then very likely the rivet will have the head knocked off.

It gives me pleasure to say that the boiler steel we ordered is to have the specification he mentioned, phosphorus .05, sulphur .03, carbon .15, and manganese .03, and I expect to have grand, good boilers.

Mr. T. P. Roberts asked attention to the following:

Whereas, It is a recognized fact that between the rights of navigators on the three navigable rivers embraced within the limits of the city of Pittsburgh, the owners of property abutting on said rivers, and the general public, there is an interminable confusion, often leading to litigation in our State and National courts; and, further,

Whereas, One prominent source of these endless disputes is the want of clear and well defined river boundaries, or high and low water lines; and,

Whereas, The "Commissioners' Established High and Low Water Lines," adopted after the surveys made in 1858, are, after the lapse of twenty-nine years, difficult to find and relocate; and,

Whereas, Such Commissioners' High and Low Lines have never been legally and firmly adopted beyond peradventure a doubt, as to the exact metes and bounds separating private property from the highways of National commerce, on our navigable rivers, in such a way as to make clear what is the right of the public to open streets or establish highways upon such reserved strips, or to make levees, wharfs or landings, or to confine the river from encroaching on its banks by means of slope walls or other shore protections, within the limits of said reservation.

Whereas, This Society is fully cognizant of the gravity of the subject looking to the rectification of the difficulties here alluded to, and of reconciling the interests, National, State and riparian, which should be harmonized by a just determination of their respective rights and privileges; nevertheless, it must occur to the thoughtful that with the growth of population and the enhancement in valuation of properties along our rivers, these questions if left to be determined by individual action will generally omit the broad interest of the public from view, and interfere to prevent wise and comprehensive improvements; and,

Whereas, In this county there was in 1858, by the establishment of high and low water lines, by authorized commissioners, a beginning made in the right direction, a system proposed, which if understood and properly enforced, might be so extended or improved upon so as to become a sound basis for the regulation of the rivers and their shores in the neighborhood of Pittsburgh, it is the sense of the Society of Engineers of Western Pennsylvania, that this is a proper time to open up a discussion of this important subject; therefore be it

Resolved, That a committee of seven members be appointed by the President of this Society, to take this matter into consideration, and that the said committee be authorized to meet and confer with Col. Wm. E. Merrill, Corps of Engineers U. S. A., in charge of the Ohio River Improvement, on the part of the United States Government, and with our county and city authorities, the Chamber of Commerce, the Coal Exchange, and with riparian owners or others interested, with a view to the re-establishment of the Commissioners' High and Low Water Lines, and their extension to McKeesport, on the Monongahela river, to Sewickley on the Ohio, and to Freeport on the Allegheny river, and to report on the advisability of any modifications or improvements of said Commissioners' lines, which the changed conditions of the rivers or extensions of the streets, etc. may seem to demand, or such modifications which would not seriously involve, or delay, or prevent the Legislative endorsement of a scheme so important to every interest concerned.

In support of his resolution for the appointment of a committee of seven members to confer with the Chamber of Commerce, and others, touching high and

low water lines in the rivers, Mr. Roberts said: I have been "billed," in the Secretary's notice of this meeting, to appear here with a paper this evening, but I am not so prepared. In regard to the motion, Mr. President, I did not expect to make any speech on it, as I supposed the preamble would be explanatory enough. The first right to rivers and navigable creeks must certainly have been the rights of navigators—the right to take canoes and flat boats along them. Following the navigators came the farmers, making their settlements along the stream, and entering their claims along the rivers.

The claims generally began at the low water mark, then back to a fixed point, then along this line and so back to the river, and thence along the low water line to the starting point, the water front being the most important of their boundaries; the main object of their claim was to secure the right and privilege of reaching water for their cattle, and also to have convenient access for loading their produce.

Nothing else, I think, was involved in that right but the right to load, and the right to have their cattle come to water. But it seems to me that now the most sacred part of any farm around Pittsburgh is the water front, and the old riparian right remains as an incubus in the march of improvement. It is so much so in the city of Pittsburgh, that the old farmers' rights still seem to act as a charm, and prevent great public improvements, or at least interfering with them very seriously, as shown in the case of the Exposition Society; and still we hear of owners to a twenty foot front city lot claiming the privilege of access to the river for their cattle.

We are also aware of the fact that our city, very unwisely, failed to secure, by proper condemnation, the rights along our rivers. For instance, on the Allegheny above Eleventh street we have no street along it, although there is a limit of high and low water lines, a vague space which the public has a lien upon, but claimed by adjacent owners. Also, there is no provision along the Monongahela for such a high way, and only in the old city proper have we any wharf of public highway on the river front.

Now it seems to me that the Commissioners' High and Low Water Lines should be re-established, and the privileges of the public with respect to them made clearly known.

I have no wish to interfere with riparian owners in their rights, but I do not think they should be left to act as they have been doing in the past, and prevent great public improvements.

In the city of Philadelphia, as William Penn neglected to provide roadways along the rivers the city has been compelled to spend, in recent years, millions of dollars in obtaining wharfage facilities, and in London they have spent millions of dollars there for "embankments" which are nothing but river streets that should have been provided for in the first place.

In this whole question there is very much involved. I see in the reports of the United States Engineers who have charge of river improvements in various parts of the country, reference to the trouble they are put to, that not even the Government can help, excepting by tedious process. These rights are so uncertain, it seems to me that it is about time that some attempt be made to clear them up, and to protect the three interests which are at stake, from conflicting with each other, viz.: the navigators, the riparian owners, and the general public.

Cities should have a well defined right to prevent the deposition of sewage in small streams or any streams where the amount of it would seriously contaminate the water. A town above on a stream will contaminate the water for the town below, and there is a conflicting of interest, and so it goes all over the country. There seems to be less known, and less established law about the lines along the rivers of our country than in any other department of legislation.

The preamble and resolution were then adopted, and the following gentlemen appointed the committee on Water Lines around Pittsburgh:

T. P. Roberts, Chairman; Wm. Martin, Wm. Metcalf, Chas. Davis, E. W. Bigelow, Chas. Ehlers, B. L. Wood, Jr.

On motion adjourned.

S. M. Wickersham,
Secretary.

ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

PITTSBURGH, April 19th, 1887.

At the regular monthly meeting, held this evening, in the Rooms of the Chamber of Commerce, twenty-five members were present, Vice President J. A. Brashear in the chair. The following gentlemen were elected members of the Society, by unanimous ballot, viz.: Messrs. Percy C. Rider, Henry D. Hibbard, Walter C. Koch, Sam. C. Weiskopf, J. S. Fielding, Emil Swanson, H. Breese, J G. Wainwright, J. A. Colby, J. P. Seymour, Geo. W. Klages, Jas. M'Dowell.

After the routine business the discussion of Mr. Thacher's paper on the Standard Specifications of the Keystone Bridge Company was resumed.

Mr. SCAIFE: I understand there is a gentleman here who will answer questions in regard to Mr. Thacher's paper. I would like to ask one question, What arrangement was made in these tests for deflection? I did not notice anything in the paper in regard to it.

Mr. OSBORN: Being Mr. Thacher's assistant, I will do the best I can. The deflection is provided for in the formula for camber, which is original with Mr. Thacher, and based partly on the theory of deflection and partly on experimental tests, and it is the only formula in any specification which takes in all the quantities that it should take in to depend upon. The amount of camber given to a bridge is in general just a little more than the deflection will amount to when the structure is fully loaded.

Mr. HUNT: I would like to make a criticism of Mr. Thacher's specifications, upon the quality of material, and especially with reference to steel. Mr. Thacher's specifications call for the compression members, bolsters, rollers, etc., to have 76,000 to 84,000 lbs. tensile strength per square inch, and for tension members, pins and built beams, 68,000 to 76,000 lbs. per square inch. This, in built beams, would include the web of the beam, which means plates, and it seems to me that this is a considerably higher specification in tensile strength than is usually demanded now by most engineers for this class of work.

Of course we all know the higher the tensile strength of steel the less ductility can reasonably be asked of it, and for tension members from 68,000 to 76,000 lbs. per square inch in tensile strength he can only demand what he has asked for in the specification, 20 per cent. elongation in 8″, and 40 per cent. reduction of area; whereas, if the material was from 60,000 to 68,000 lbs. he could reasonably ask what is called for in the table below—22 per cent. of elongation with 44 per cent. reduction.

It is my judgment that the extra ductility in such material far supersedes in value, for structural material, a few thousand pounds extra of tensile strength, and my opinion is based upon the actual punishment that steel used in structural material gets in actual use, rather than upon theoretical claims, for I will admit that the engineer in working out the details of his structures from the strain sheet can more advantageously use the higher tensile strength material, but it seems to me that this is one of the cases where theory and practice are a little against each other. This is so apparently, when it is a fact that a bar of steel as large in section as 8x2 can be struck at a cold temperature a blow with a hundred pound drop ball from a height of ten feet, and it will break; snap it right through like a pipe stem, as will be proven by the paper of Mr. Ramsey at our next meeting; while it is true that under ordinary conditions, ordinary temperatures, a blow or impact will break a high steel very much quicker and very much easier than it will a steel of lower tensile strength. What I mean by a high steel, is material over 70,000 lbs. tensile strength, and such material is very much more brittle, particularly under impact, and in view of this it seems to me that we can wisely use the lower strength material.

There is still a great difference of opinion among engineers on this point. Some engineers are requiring as high as 80,000 lbs. tensile strength for the same material that Mr. Thacher asks from 68,000 to 76,000, whereas many others are becoming of the opinion which I have just voiced that steel of 60,000 to 68,000 lbs. tensile strength, gives the best qualities for structural use. In this I do not wish to be understood as meaning that the material shall be actually between the figures of just 60,000 to 68,000 lbs. I know there is a great deal of "refinement," which means the refinement of nonsense. It is a fact that taking a piece of steel of the most homogeneous and uniform character that can be found, and preparing test strips as finely as can be done, and test them as uniformly as possible; it is a fact that you can rarely get the several strips to test within a thousand pounds of each other in tensile strength per square inch.

But rarely can you get the percentage of elongation to come within one per cent. so that a demand in the specification which would accept material which should be down to the hundreds of pounds and reject, if there was a difference of a few hundred pounds one way or the other from the specification, is, as I have said, in my judgment, nonsense. It is such "refinement" as the chemists have when they figure their percentages of the elements they find, down to the one ten-thousandths of a per cent. I remember once a criticism of a chemical result, where the chemist figured down to the one-fifth decimal place, that another chemist analyzed the same substance and stated he could not tell if the first man was correct on the fourth or fifth place, but was sure he was wrong on the second.

Mr. Buchanan: Mr. Thacher said in his reading the other night that he ignored the effect of centrifugal force. I do not know whether I am competent to discuss the matter with him, but in looking over some data, I find that when the curve is as sharp as ten degrees, which occurs in the alignment of a great many railroads, the horizontal stress produced by centrifugal force is equal to thirty per cent. of the live load. As in the case of a ninety-two ton engine this would amount to more than double the stress provided for under the requirements of these specifications, in spans of average length, it strikes me it would be well to consider the point. Also, in proportioning the truss or girder it would seem to be important in some cases that this force should be considered, for in the case of a deck girder bridge with girders six and one-half feet apart, the outer girder would have to carry sixty-six per cent. of the entire load when the train was running at a speed of fifty miles per hour.

Mr. Osborn: I think Mr. Thacher's idea was to exclude the centrifugal force altogether, excepting in cases where the centrifugal stress amounts to more than the wind stress, when he makes use of it; the idea being, that you will not likely be running a train at full speed over a bridge when the wind is strong enough to overturn the train.

Mr. Buchanan: Mr. Hunt, in speaking of the 8x2 bar breaking, said it did so under a one hundred pound weight falling ten feet. I would like to ask him if the surface of that bar was as it came from the rolls, or if it had been tampered with?

Mr. Hunt: In the case I refer to the bar had been nicked, or rather had the tool mark of the planer nearly ⅛″ deep. It was not the nick of a chisel, but the tool mark of the planer on the 8″ face of the bar. That is one of the peculiarities of steel. If you once start a nick in it, it is gone; this nick once made and the drop or impact applied, it will crack like a pane of glass, and in that iron is superior to steel. Iron will stand impact much better than steel; the crack seems to be stopped by the non-homogeneity of the material.

Mr. Barnes: I should like to turn that part of the discussion to practical account. We have in our steel works yard a number of cobbles. They vary from 8x9 down to 1¾x13, some ten feet long, some 30 feet. Now I should like to inquire down to what point of temperature they should be cooled in order to break easily. I will say further, that for certain reasons, we have thought it worth while for the time being to let them accumulate. We have perhaps 15 or 20 of them on which we have utilized some spare labor, nicking them deeply with a cold chisel to a depth of ⅜″. We have spent a good deal of time sledging them, but they are stubborn things to break. Now, to what temperature should they be cooled in order to break them with the same success as the 8x2 bar which was broken by a hundred pound weight falling through ten feet?

Mr. Hunt: I do not know just what to recommend, but would suggest down to a temperature of 3800 degrees Fahrenheit, or one that would easily melt the steel up.

Mr. Barnes: That is dodging the question. In order to melt them we have to cut them up, some of them. If we had them cut we could melt them without difficulty. We have had a stubborn task to cut them, and if I can get any light I shall be very glad, indeed.

Mr. Koch: I would like to say something about this steel business, and first of all to that question of plates for bridges. I remember many years ago when they first made steel—that is, away back in the 60s—when a specification called for 40 tons ultimate, with twelve per cent. elongation—that is about 90,000 lbs. I think. We started in to make it, and after a great deal of trouble we succeeded in doing it, but I called the engineers' attention to the fact that they simply had rotten material. Said I, you have just what the specification calls for, but you have rotten material. You think in order to make this hard steel we will take and put in carbon, but carbon is too expensive, and means spiegel. I said, it is far cheaper to put in phosphorus. Of course your bridge will stand, but after a time it will crystallize, and then it will go down; and he found I was right. He then brought down his specification to 70,000 lbs.

Personally, as far as I am concerned, if a bridge maker comes to me with a specification, and calls for anything over 70,000 lbs., I say to him that he can go elsewhere, for I decline to make it. When you make anything over 70,000 lbs you are getting up to the danger limit. My opinion is, that the best steel that is made, is between the limits Captain Hunt

mentions, between 60,000 and 70,000 lbs. tensile strength. You then get a good material, having from twenty per cent. elongation in 8″ up to thirty per cent. in 10″. I think myself that as yet we are very far from what I consider a perfect steel material for bridges. I want some day or other to make a steel that will give us a high elastic limit, that is when the impact comes against it, it will have sufficient force to resist.

A perfect steel is that which will give a high elastic limit, that will show about 60,000 to 70,000 lbs. ultimate, with an elongation of 25 per cent. I think the trouble with steel is not in raising the elastic limit, for I believe that is too low. How far other gentlemen will agree with me I do not know, but my impression is, that the true strength of steel lies in the elastic limit. If you get this, and also a steel that will stand 60,000 to 70,000 lbs. tensile, I think you will get the very finest kind of steel for purposes of bridges, boilers or anything else.

The great trouble is, to keep down the impure elements in the steel, that is to say, the phosphorus and sulphur, and to get the manganese up. For instance, all the steel for the Indus Bridge contained from .70 to .80 manganese. That is higher than is used in this country, where it varies from .4 to .6, but it filled that specification. If the phosphorus and sulphur are put under .05, and we get about as much manganese as we like, the steel is sure to be of a good quality. In the case cited the steel cost money, but they were willing to pay for it, and they got very fine material.

About breaking cobbles, that is a thing I should like to know about myself, for it is something we cannot help. About nicking steel, my experience is very curious. Sometimes I have taken steel bars of all thicknesses, all kinds of steel bars and steel railway axles, with steel bars of all shapes and sizes, cobbles and everything else. Sometimes if you nick them, and nick them slightly, and drop a weight, the bar will break without trouble, and then again it will not break at all. I have known steel to break at the first blow if only slightly nicked, but if you take a piece of the same steel and nick it deeper it will hold together. You cannot always tell about this, and even if nicked an inch or two deeper it will bend and twist up like a snake. As to temperature, I have tried all sorts of cobbles on the coldest day, and I rarely had any more success at zero than in summer time, but it is a curious fact, that a cobble or bar if only slightly nicked will often break easily, while the same bar, if deeply nicked, will give you the mischief to break.

Mr. William Metcalf: I suggest that a reason perhaps for that is, that a very slight nick made with a sharp chisel will simply break the surface without disturbing the steel, but when you dig into it for one-half inch or an inch the nick becomes large and it compresses the steel in front of it, and then you get something difficult to break. Mr. Koch wants high elastic limit. I suggest that he apply to the Army and Navy Ordnance Board or the Cambria Steel Co. They will give it to him, that is, they will tell him how they do it. I do not know whether it will cost more than manganese or not, for I never tried it.

Mr. Koch: By tempering, the final fracture will come up to 80,000 lbs. We tempered gun coil, and that was all right. We got a high elastic limit, and also a high ultimate steel. The ultimate for gun coil was as high as 96,000 lbs. per square inch.

Mr. Miller: In regard to these gentlemen talking about not being able to break a cobble, I have tried my hand at a 15″ shaft, steel shaft made by Krupp. There was a little crack or tear about 2″ long in it. I put it under a ten ton hammer, and I expected to have a great deal of trouble to break it. I rested it about six inches off the anvil and blocked it up each side. I hit it one blow, and it broke as if it had been made out of a piece of clay. It broke so easily that it took away half of the anvil. It was 15″ in diameter. The crack was about 2″ long. Regarding cobbles, I have tried a few of them, and have had as a rule no trouble in breaking. I have tried some of those other cobbles with the carbon about .16, tensile strength about 60,000 lbs., and I have been very much like this gentleman here—I have sometimes found some that I could not break, but this was American steel, and as I am an American I thought that was the case with all American steel. I find that American steel is tougher than what we get from the other side.

Mr. Barnes: With regard to cracks in shafts, I was deeply interested, for this reason: In our works we have a cracked shaft. It is steel, 14″ diameter. It is on one of the main journals of a reversing engine, run with 110 lbs. of steam. That crack has been there about two months. It extends half way around the shaft, and appears to be about ¼″ deep. It is a very fine hair-like crack, and so far as we can tell the shaft is not working on the crack at all—if it had been it would have gone long ago. The crack is extending some. It is an American shaft, which will not break possibly as easily as the German shaft. I am interested in hearing what Mr. Miller says. We are holding on to the shaft, running the engine, believing it is all right for the present, although we have ordered a new one.

Mr. Metcalf: I would suggest that if he will drill a small hole at the end of that crack it will stop it for a long time. We put up last summer a ten ton hammer with one of these beautiful arrangements, a key in the rod to hold it, and at the end of the first two

weeks of running we not only knocked several parts to pieces, but we cracked that ram through the key-hole, starting the crack at the bottom, and extending on both sides. We immediately ordered a new ram, and got a good one this time because it has no key-hole in it. To try to make this ram run until we could get a new one, we drilled small holes at the ends of the cracks, and have been running with the cracked ram for six months, and working as hard as we can, and only one crack has passed the hole, and we are using it every day. It is still holding on and working very well. I merely suggest this as a possible means by which you can stop that crack from extending. The hole should be about ¼″ diameter, and about the depth of the crack. It will stop it until you get your new shaft.

Mr. Brashear: Do you notice any dust working out of the crack?

Mr. Barnes: No, sir, there is no dust coming out of it. Indeed, if there was any it would be so mixed with oil as to elude detection in that way.

Mr. Brashear: Where there are any synchronous vibrations in the metal a small hole so drilled will frequently stop them, but if the vibration is kept up, and that does not destroy it, the crack seems to travel on without any interruption. The question is very interesting in connection with the shaft.

Mr. Hunt: I would like to ask Mr. Metcalf what is his theory as to the holes stopping the crack working on?

Mr. Metcalf: I do not know whether I have any theory. It is one of those ideas I picked up from some good mechanic, from whom many a good idea can be obtained. It was an old trick in such cases. I presume that it checks the synchronous vibrations. You drill a round, small hole there, and you have destroyed the contiguity of very many of the crystals in your piece of steel. If you start a rupture of the crystals in any crystalline body and set up a series of continuous vibrations, it will go to pieces, just as if you take a piece of cloth or paper, which, while it may be very stiff, once started it will tear very easily. It seems to me all crystalline bodies break in the same way. If you drill a round, smooth hole at the end of the crack, not too large, not to destroy the strength of the material, the small hole will cut off the break in the first place, and then the hole being round itself it is like a fillet at the corner. The vibrations of the steel go around that hole, and are necessarily distributed in the walls of that hole, and there is no place where the crack can again be started.

We had a case some years ago with a three ton hammer, where we were talked into using a key (which I think is the most abominable invention ever made). We broke three rods in one year, and finally broke the ram. Then we had the ram bored out, and tapered the hole, turned the rod to the same taper and fitted it in. That hammer has been doing good work ever since on hard steel, and it looks as if the rod, so fitted, will stay there. I have made the same change on this ten ton hammer, the ram is made heavier by one-half ton than the original, and I propose to fasten the rod and ram together in the same manner, and it will never get loose. I think neither the rod nor ram will ever break. But the key driven in there started the crack, and it kept on going until we drilled the holes and stopped it.

Mr. Brashear: I think that will be found to have a good effect. We all know the effect on bells. The whole theory depends on breaking up the synchronous vibrations, otherwise destruction will result.

Mr. Barnes: In the case of the shaft, the crack extends into the shaft away from the surface. You cannot prevent the extension of the crack inward, but I am not prepared to say it will not be an advantage to drill at the ends of the crack to an equal depth. It is possible, however, the depth may be considerably greater than I imagine. My feeling is, that the crack is not very deep, and I am very hopeful it will hold on at least until we get the new shaft, which is well on now towards completion.

Mr. Brashear: It would be a very pretty thing if we could look into iron as we can look into glass

Mr. Miller: I think, Mr. Chairman, that Mr. Metcalf struck a little chord there that is very familiar to me in regard to making a fillet in that little hole. I can endorse Mr. Metcalf's theory about these little holes. I have had it done, and it proved very effective. And the word "fillet" is very suggestive, for I think a great many shafts, both iron and steel, are broken for the want of a fillet, having a square corner or bearing—that the shaft, I think, is just as well if it was nicked with a very fine chisel. I certainly think if all shafts, either iron or steel, had a good large fillet in the corners, there would not be so many broken shafts. I have seen it in sugar mill shafts, where some with a fillet never broke, while others would be broken in a very short time that were not so supplied—broke the first season.

There is a mill here in Pittsburgh, that has broken two shafts, and unfortunately I made one of them. I went to see it, and it was good iron, but I found it had a square corner, and I also called attention to the fact that the journal was too small for the size of the shaft, and you have a sharp corner. Well, says he, what are we to do? Well, I replied, if in your next shaft you will remedy this trouble, I do not think you will break it. I put up another shaft. It is now running and has been for some years.

Mr. Brashear: I can readily see how that will hold good with a cast shaft, for the molecules cannot

form, and get around a square corner, but in the case of a worked shaft, by hammering, I cannot see that it would make much difference.

Mr. Miller: It is not the hammering, but the cutting with the tools.

Mr. Metcalf: We had a customer in the West who used saw mill shafts, and they appeared to last but a very few weeks. We had frequent orders from him for another shaft They complained of the shafts breaking, and finally the user of the shaft himself got tired of ordering, and said he would not buy any more, and would not pay for the last one he broke. That was made of good steel, not so low as the structural steel. The shafts were hammered to the shape necessary to form the crank, and the crank part was hammered on solid, and sent in that shape. They had turned the journal of the crank up perfectly square, and left it in that manner, and they had broken a dozen shafts in the course of two or three years. For curiosity I took the journal right off from one that had been sent us, and sent it to the Keystone Bridge Co, who tested it, and it gave 95,000 lbs. tensile strength. It was about 55 or 60 carbon, crucible steel shaft, and would not have broken if the parties had put a fillet in the corner of the crank. We wrote back, and told them the cause, saying it was not our business, but if they used any more steel shafts, if they would have the gumption to put fillets in the corners they would not break, and unless this was done the shafts would break in the manner they described, every time. I would not trust any steel shaft, no matter how large, if it were turned up with sharp corners. They will break in some such corner.

On motion, society adjourned.

S. M. Wickersham,
Secretary.

ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

Society came to order 8.30 P. M. June 21st, 1887, in their rooms, Penn Building, thirty-two members present. J. G. Siebeneck in the chair. The usual routine business was transacted, and Emmerich A. Werner, of Keystone Bridge Works, recommended by E. Thacher and Emil Swensson, and endorsed by the Board, was duly elected a member of the Society. Mr. J. A. Brashear read the following paper on the "Parallelometer."

A GRAVITY PARALLELOMETER.

One of the most difficult tasks the practical optician meets with, is the production of *parallel* surfaces. In the preparation of such surfaces for polishing, great care is necessary to approximate as closely as possible to parallelism, especially in the "fining" process, in order to leave the smallest amount of correction for the polisher to do. Ordinary calipers may be used for the detection of errors in the glass plate, and it is astonishing how small an error may be determined by a practiced hand, but the least deviation from a line drawn through the centre of the plate and the axis of the calipers will seriously affect the resultant reading. The spherometer and micrometer caliper are also readily adapted to the study of errors from parallelism, and the accuracy of these methods is not here called into question, but in our work we have found that they consumed considerable time in going around the circumference of a plate. Personal errors of *contact* are also brought into the problem. An instrument to work by *gravity* alone was suggested by one of my assistants—Mr. George Klages—who constructed for me a very excellent design, which has been further improved in the present instrument. The device is quite simple, as will be seen by the following description:

A is the base plate of the apparatus, in which are seen three conical pins b b' b''.

One of these pins b is made of hardened steel, rounded slightly on the apex of the cone, and nicely polished so as not to scratch the glass. This pin is placed directly under the point of the pendulum p, when this pendulum is vertical, though no great exactness in this regard is essential.

The other two pins b' b'' are placed in the slides s s, and can be moved to any position to suit large or small plates. It is not essential that these points form an equilateral triangle with the point b, an approximation being all that is necessary.

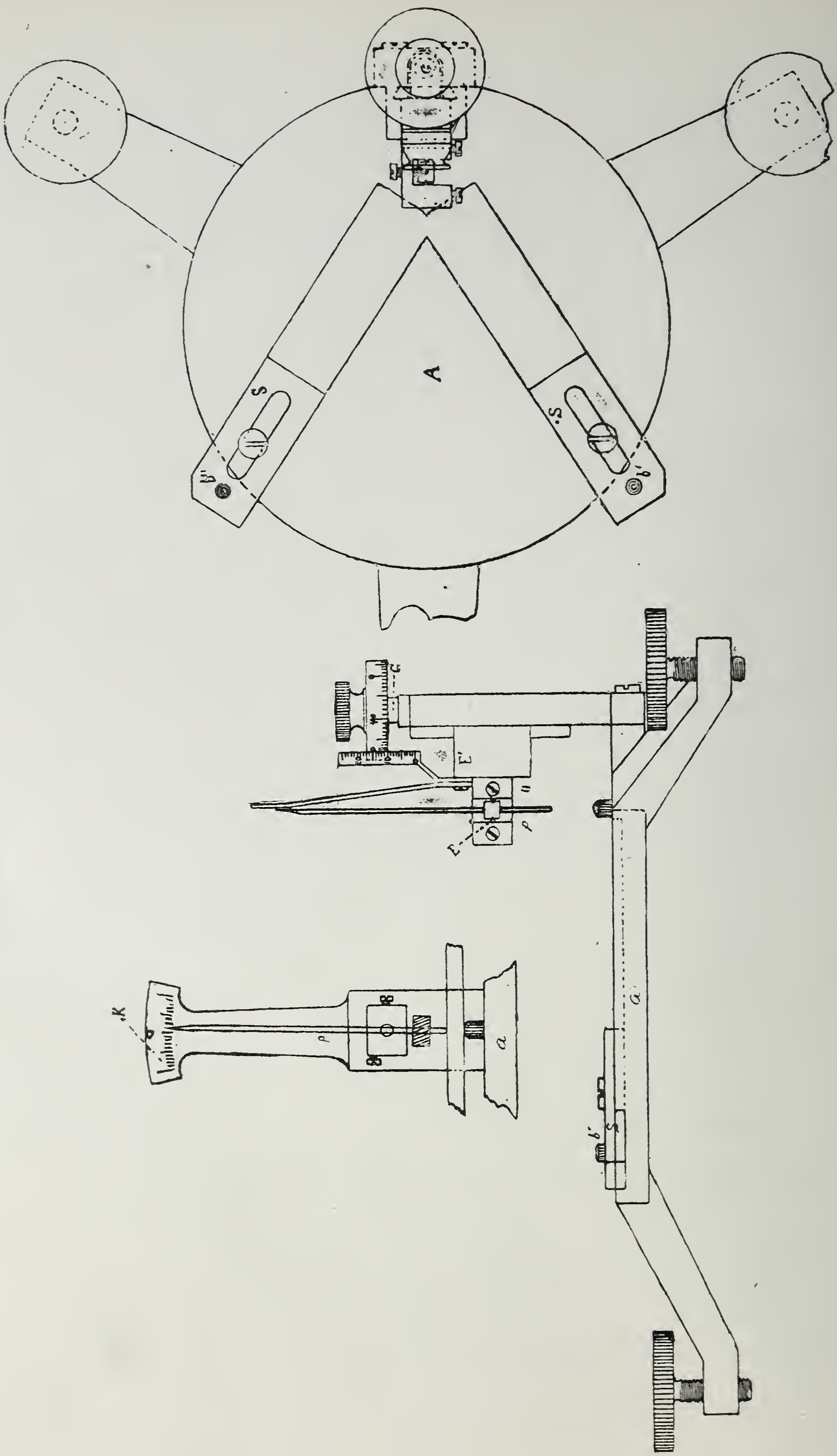
P is a pendulum supported on two delicate centres c c'. This is the most important part of the instrument. The pendulum and its supports can be moved in a vertical direction by the screw g, which in this case is operated similarly to a micrometer screw, so as to make up for all lost motion, though for the use it is intended, this precaution is really unnecessary. The head of the screw g is divided for mere convenience of reference. The lower point of the pendulum rod is slightly rounded, and is polished so that only one point shall come in contact with the glass plate. The upper portion of the pendulum is made very light, and at the top is drawn into a fine point to serve as an index to the divided arc k.

It will be noticed that the motion of the pendulum is arrested in one direction by the screw l, a very necessary precaution, and the glass plate being tested, is *always* rotated under the pendulum in the direction that it is free to move.

The divided arc k gives at once the reading of the swing of the upper end of the pendulum p. As this end of the pendulum may be many times longer than the lower end, almost any degree of delicacy may be obtained, but in practice we find from five to eight times sufficient for our use, and with it made of such proportions, there is no difficulty in quickly detecting errors less than $\frac{1}{10000}$ of an inch. It must be understood, however, that it is not quantity we wish to measure, only so far as we desire to know whether there is an appreciable error in a plate, and as the *first* movement of the pendulum is, "to all intents and purposes," in a tangential line to a radius which is vertical with respect to the plate, it can be readily seen that a very minute quantity or error in a plate can be detected. To make this plain, however, it is only necessary to give in a few words the process used in testing a plate for parallelism.

First. The two points b' b'' are set in a convenient position in reference to the plate to be tested.

Second. By the screw g the point of the pendulum p is brought down to the thinnest part of the plate previously placed beneath it. This is readily found by running the screw down a little lower than the *supposed* thinnest part of the plate, then by rotating the plate, the index i will at once tell us where the thinnest part is. The screw is then set so that the


A
S
S

index is vertical. It will now readily be seen that every part of the plate can be quickly studied by rotating it under the point of the pendulum, its errors marked on it with a soft lead pencil, and the proper corrections applied. The only drawback to its use is in minute dust particles getting under the point of the pendulum, but as these dust particles cause a *rapid* movement of the index, this trouble is easily eliminated in practice. And we have found this little device a most admirable help in the preparation of parallel surfaces.

Of course the final testing must be done by cuticle optical means, as the polishing proceeds, and here we must fall back on those *refined* instruments, the *human hands*, directed by the eye and brain. Yet so much has this device assisted in our work, that my assistant, Mr. James McDonell, has produced plates whose error from parallelism has been so small as one second of arc; and in one instance the error was so minute that a mathematical line drawn from the two surfaces would not unite in five miles, the plates being about 6 ^{mm} thick, or in other words, the deviation from parallelism in the plates in question was $\frac{1}{100000}$ of an inch. I do not wish my testimony to be taken here, but I give you the figures as given me by some of the most eminent physicists of our land, viz.: A set of plates made for Prof. A. A. Michelson, greatest error 1.5″. A set made for Prof. Anthony, of Cornell University, greatest error 1.0″. For Gen. Hazen, no error detected. The evidences could be multiplied, but these are sufficient. Our aim is the best work, or none.

After the reading of this paper the discussion of Mr. Danse's paper on "Fire-proof Buildings" was continued, viz.:

Mr. Hunt: I would call the attention of the members of the Society to the fact, with reference to the paper that was read at the last meeting, that the writer stated that one of the buildings which was in one sense a strictly fire-proof structure, is one which is now a pile of ruins. I refer to the First National Bank building, at the corner of Fifth avenue and Wood street. It has been shown that it was fire-proof only in part.

Mr. Barnes: I would like to ask for information in regard to the distinction brought out between an incombustible and a fire-proof building. This First National Bank was spoken of as being incombustible, but yet as not being fire-proof. Wherein lies the difference between these two terms?

Mr. Siebeneck: I believe that the writer stated that an incombustible building was a structure that was made from incombustible material, that would not burn, and yet was not fire-proof; that good timber, properly surrounded, is far more safe than iron would be. I only cursorily read over the paper, but I noticed this distinction. It was a curious one to me.

Mr. Hunt: It seemed to me that there was a different meaning from this in the paper, as to what was the distinction between an incombustible and a fire-proof building. Now it is a fact, there is a large pile of iron extending up above the horizon where a building was, that did not burn, because it was practically incombustible, or rather the material that was in it. In that sense the walls of the building, that which actually made the building, were incombustible, and at the same time the building was not fire-proof, from the fact that it was so constructed that the entire wood work could burn and could practically be destroyed without having the walls fall down. A fire-proof building is one that, if a fire should start in the wood work necessary about any large building it would be so disconnected from the rest of the building that the fire would be only in the one apartment where it started; that it could not go on to other rooms. That was my idea as to what he meant by the fire-proof building, and that the incombustible building was one where the walls might stand up, as in the case already cited, and still it would not be fire-proof.

Mr. Siebeneck here read an extract from the paper of Mr. Danse, bearing on this subject.

Mr. Roberts: In regard to the expansion of the floor beams, I made inquiries myself, but could get no information on the subject. Does anyone here know about that?

Looking up from the street the arches seemed to be intact. I have no doubt the uppermost floor was very highly heated, so that I cannot see there was any trouble from expansion. In that building, probably there was some arrangement to take up the expansion of the floor beams.

It seemed to be a pretty good fire-proof building from the fourth floor down. The mansard roof was of frame, and reminded me of a building on Broadway, New York, that cost two and a half millions of dollars (the Equitable Life Assurance Society building), and the eighth story, I was very much surprised to find, was all wood. All below was incombustible, and I thought it was a great mistake to leave that fire box on the top of the building. It also appears that the principal damage in the First National Bank building was from the water. The Bank's room below, and the stores, would not have been injured except from this. The fire was kept in the upper rooms; but had the floors of these rooms been water-proof the lower stories would have been all right. I believe that it is practiced in some places to make floors with gutters around the edges of the walls for the purpose of carrying off the water in case of a fire. They should have buildings not only fire-proof, but water-proof.

Mr. Wickersham: I am very sorry that Mr. Danse is not here to explain what he meant by incombustible and fire-proof buildings. I think that what he means, as explained to me, was very sensible. There is nothing, he admits, but can be destroyed by heat, if the heat is raised sufficiently. There is no material we have but what can be worked and molded under heat, therefore nothing we have, but what can be destroyed, if enough heat is applied.

But his idea was to build a fire-proof building, even of wood, which is very destructible, of which we make floors, but to protect that wood so that in all human probability no heat will reach it sufficient to burn it. Then you have an indestructible building made of very destructible material, but he does away with wood, and uses what he calls porous tile. With this porous tile he covers all the wood work necessary to be used, covering it so well and spiking it to the wood, covering the heads of the spikes and nails, or whatever he uses, in such a way that it is not probable that heat enough will be raised from any source to affect it..

If he succeeds in that, he has a fire-proof building, although the main components of that building are destructible by fire. If the building is built of iron, with iron girders and iron joists, it is protected in the same way as shown in the cut in the paper of Mr. Danse. Not a particle of the iron is exposed to the heat, therefore there will not be enough heat to warp the iron and disarrange it, and the building is practically indestructible. Parts of the building may be destroyed, but if the whole is built in that way, of either wood or iron, the fire cannot spread from one part to another, and therefore it is indestructible. That is Mr. Danse's idea, I think, as near as I can state it in his absence.

Mr. Roberts here exhibited a specimen of the porous tile referred to by Mr. Danse in his paper, which was examined by the members.

Mr. Koch: Suppose that terra cotta is exposed to a very high heat, and then the hose from a fire engine is turned on it, what will be the effect? Mr. Danse did not mention that in his paper.

Mr. Roberts: Mr. Danse said that the material had been tested in that way.

At 10 o'clock Society adjourned.

S. M. Wickersham,
Secretary.

JOINT EXCURSION

OF THE

Engineers' Society of Western Pennsylvania, the Society of Architects, and the Amateurs' Society of Photography, *via* Steamer to Brownsville and Return,

J·U·N·E 24·T·H, 1·8·8·7.

☞ ***The boat will leave the wharf above Market Street at 8 A. M. sharp, Friday, June 24th, returning about 10 P. M. Single tickets will not exceed in cost $3.00, including all expenses for meals, music, &c.***

[*Memoranda of the Monongahela River*.—The Monongahela river is formed by the junction of the West Fork and Tygart Valley rivers, in West Virgina. From the junction to Pittsburgh the distance is 129.7 miles; including the Tygart Valley, the entire length of the river is 264.7 miles. Area of the valley about 7,000 square miles (about one-half that of the basin of the Allegheny river). The fall of the river from Morgantown, West Va., 102 miles to Pittsburgh, is 89.97 feet, or about 0.8 feet per mile. From Morgantown to forks of river the fall is nearly 2 feet per mile. (Elevation of low water at Pittsburgh, 699.16 feet above mean tide.) The Youghiogheny, Cheat, and Tygart Valley rivers, all mountain streams, furnish the great bulk of its perennial supply of water. The Monongahela is called the "islandless river," from the fact that it contains no islands within the limits of Pennsylvania, its present navigable portion, (above that only two insignificant islands occur to the Forks.) It is improved by a system of locks and dams. Of these, Nos. 1 to 7 inclusive were constructed and are operated and maintained by the Monongahela Navigation Company. No. 8 dam, at the mouth of Dunkard Creek, Greene county, Pa., and 2 miles below the mouth of the Cheat, is now being constructed by the United States. No. 9, five miles below Morgantown, West Va., is already completed, and is the property of the United States. Navigation to Morgantown (the seat of the University of West Va.) will probably be open during the year 1888. The name "Monongahela" was given by the Indians to this river. The word signifies "muddy water." "Allegheny," in the Seneca dialect, and "Ohio," in the Mingo tongue, signify "clear" or "beautiful" water. The Indians evidently regarded the Allegheny as the prolongation of the Ohio. So also the French, when they translated the names, applied "*La Belle Riviere*" indiscriminately to both of these rivers. Allegheny river is 325 miles long. The tonnage represented in the boats and barges and steamers engaged in the coal trade from the Monongahela river, viz.:—About 140 steamers and 3,000 boats and barges, representing a greater carrying capacity than the aggregate tonnage of all the rest of the Mississippi Valley. About 100 coal mines are in active operation in the lower four pools (none above the 4th, as yet), which give employment to about 9,000 miners.

POINTS OF INTEREST BETWEEN PITTSBURGH AND BROWNSVILLE.

First Pool.

Leaving Lock No. 1 the first bridge is the recently completed structure of the Monongahela Connecting R. R., extending from the Eliza Furnace on the right to the American Iron Works on the left bank of the river. After leaving Hazelwood and Glenwood we pass under the B. & O. (Wheeling extension) R. R. bridge. The railroad leaves the river for the West by Streets Run. A mile above the bridge on the left bank is the City Farm (near Homestead). Here we pass under the Pittsburgh, McKeesport & Youghiogheny R. R. bridge, with its notably long approaching

trestles. The Pittsburgh city boundary strikes the river just below the right shore end of this bridge about 9 miles from the Point, or mouth of the river). The new Homestead Steel Works on the left, the Carrie Furnace, and the Miller Forge on the right, are passed below the town of Braddock. Braddock's new water works, and the Edgar Thomson Steel Works (particularly the latter), with its numerous furnaces, form striking objects here. Braddock's army crossed the river to meet the French and Indians in ambuscade at a point about opposite furnace E, July 9th, 1755, upon which occasion about 800 out of Braddock's army of 1800 were killed or wounded. The worst fighting was done on the hillside at the lower end of the Steel Works' immense inclosure, and the ground is clearly visible from the steamer. Here Washington *dug up* his little hatchet for the first time. Through these great works there passes the P., McK. & Y. (Vanderbilt) road, on the river bank. Next the Baltimore & Ohio, and on the hillside behind them the Penn'a R. R. main line—three great rival trunk line roads. Passing up the left bank of the river to West Brownsville is the Pittsburgh, Virginia & Charleston R. R. (it crosses the river near Brownsville and terminates at Uniontown). Just above the mouth of Turtle Creek we pass under the bridge connecting the P., V. & C. with the P. R. R., and here enter Lock No. 2 at the village of Port Perry, 12 miles above Pittsburgh.

Pool No. 2.

Two miles above Lock 2, on the left are seen the works of the New Duquesne Steel Works, and at 16 miles above Pittsburgh we have McKeesport and the mouth of the Youghiogheny river. The difference in the color of the mingling waters presents somewhat the contrast of the "blue and the gray." The B. & O. and the P., McK. & Y. Railroads pass up the Yough to Connellsville, etc., so that above this point there is no road on the right bank. On the left, above McKeesport, is Dravosburg and other mining towns. At 22 miles above Pittsburgh we pass between the towns of Elizabeth on the right, and East and West Elizabeth on the left bank, and 2½ miles above enter Lock No. 3.

Pool No. 3.

A succession of coal works is seen, and at 31 miles above Pittsburgh we pass the thriving town of Monongahela City. Above this we have Webster on the right, and at 41 miles reach Lock No. 4.

Pool No. 4.

Belle Vernon is seen on the right 2 miles above the lock, and on the same side, above it about 2 miles, Fayette City. At 52 miles we pass California, the seat of one of the State Normal Schools (average attendance, about 500). Just above the P., V. & C. R. R. bridge over the river, and 57 miles above Pittsburgh (2½ miles below Lock No. 5), we reach Brownsville.

BROWNSVILLE.

Brownsville occupies both banks and the hillsides of Dunlap's Creek, over which is to be seen a unique cast-iron elliptical tubular arch bridge—the only one of the kind in America of considerable span. It was built by the General Government in 1822, to carry the great National road. The greatest period in the glory of Brownsville passed away with the days of stage coaching. The photographers will here find good subjects for pictures, among which may be mentioned —The National Bridge; the landing, with its quaint walls supporting buildings; the old market house; the United States Hotel; the ivy-crowned tower of the Episcopal Church, and its handsome churchyard; several terraced and walled residences. The view from the rear entrance gate of Mr. Jeffries' residence looking down upon the river, and the bridge crossing it, is very fine. Mr. Jeffries' house and garden occupy the position of Red Stone Old Fort, a prominent post in early times, and in his garden are the tombstones of two of George Washington's nephews, viz.:—Archibald and John H. Washington; also that of their friend Edward B. Machen, of South Carolina. These three young men were poisoned in Brownsville in April, 1818, while on their way west with a lot of slaves. Opposite the steamboat landing in West Brownsville is the birthplace of James G. Blaine; the old house is still standing.

On the return trip the United Societies are invited to stop at Lock No. 4, by Major Thomas McGowan, and hold an informal meeting in the pleasant new hall of the Union Literary Society of that place. The Photographers can also find several good subjects for pictures at Lock No. 4.

THOS. P. ROBERTS,
JAMES BENNY,
W. L. SCAIFE,
L. O. DANSE,
WM. MILLER,
Committee on Excursion.

ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

Society met at their rooms, Penn Building, Tuesday, September 20th, 1887, at 8 o'clock P. M., twenty three members and two visitors present, President Dempster in the chair. Marcelin Scaife, Chas. Bailey and W. W. Fortune were duly elected to membership. Thos. P. Roberts read the following:

Our society is now in the eighth year of its existence, and in point of membership and average attendance is in a flourishing condition. Financially, I believe, we are well off, and have pleasant meeting rooms, an excellent library of standard works of reference, engineering periodicals, etc. So, then, as far as these outward and visible signs betoken, the Engineers' Society of Western Pennsylvania is not surpassed by any similar organization in the country. We can congratulate ourselves in the further fact that in certain departments of engineering, fine art, as, for instance, chemico metallurgy, our society takes a very prominent position; the lead, certainly, in all that pertains to the manufacture of and uses of steel in construction. So, also, in the refinement of astronomical instrument manufacture, no one disputes the wide-world fame we can herald in our membership. However, this is scarcely the time to review the history and achievements of the society; if it were, it would be easy to speak of the departments of rail-making, railroad construction, management and maintenance, bridge-building and inspection, foundations and masonry, engine and boiler-making and management, water works sewerage, natural gas—chemically and employment of—in every one of which we have accomplished experts among us, some of whom enjoy a national reputation. But we must defer this self-laudation to the tenth anniversary of our corporate existence, and I only refer to these things as the happy boy does who jingles his first two half-dollars in his pocket, not because he is about to squander them, but merely to excite the pleasure which a realistic knowledge affords him, that he has them to squander when he feels like it.

But it is not enough for us to revel in past glories; we should be looking ahead for fresh laurels, and it occurred to me that this opening night of another season, when we have no regular paper for discussion, would be a good time to revamp our affairs, and stir up, if possible, a spirit favorable to progress.

Our organization is unique among the engineering societies. It is not really an association of civil engineers, in the ordinary acceptance of that term, for, in addition to engineers, military, civil, mechanical, mining, metallurgical and hydraulic, we have among us manufacturers of the material which the engineers employ, and chemists, and other experts who test the manufacturers' products, and who are not afraid to discuss them right here in our meetings; besides these we have also representatives of the men who furnish the means to employ the engineers, for, also, it is true we cannot all be engineers.

This is by no means a combination of heterogeneous interests, likely to result only in the building of a tower of Babel; and, after all, I have never heard there was anything wrong with that tower itself; the bricks were not hard burnt, it is true, but the cement passed inspection all the time.

When one of our skillful associates condemns steel for rivets in boilers, or another points out the means to prevent bridge disasters, it is not so much for the benefit of his class-fellows, but rather more for the benefit of those who organize the projects and pay for the boilers and bridges. Intelligence is much more widely diffused among business men than it was in the past generation. The tendency among our successful business men is to devote their leisure time to some department of science. We have a popular Fifth avenue merchant ranked among the microscopials; a Presbyterian divine of the East End standing among the highest as an entomological; so others are physicists, and among these we have caught in our society the amateur engineers. It was not so fifty years ago. I heard my father remark that in 1832, when he was sent to take charge of the surveys for the Portage railroad cross the Allegheny mountains, in the entire list of engineers in this State only six were competent to lay out railway curves, the canal work previous to that time not having required the work of such "specialists." Those were the days when a transit man looked down upon a leveler as a first lieutenant of a saucy English frigate regarded a midshipman; the period when years of experience were required with plus and minus sights before it would be safe for candidates to seek the privilege of touching the sacred tangent screw, or attempt to solve the mystery of the vernier plate. But in these days the use of instruments is taught in almost every school, and the public has learned to distinguish an engineer with or without a pair of high top boots.

If we were called upon to restrict the membership and scope of our society, where would we begin to draw the line? Would we aim to make it an association of mining, or mechanical, hydraulic, electrical, or gas engineers, or simply old-fashioned civil engineers and surveyors? There was a time when the civil engineer coped more or less successfully with every division of his art; still, the evolution in his business is not without its parallel in other professional occupations. All, or nearly all the sub-divisions of the great profession possess independent national organizations, which is well for them; but a national organization on the basis of our society might not flourish, because its meetings could not be held frequently enough to develop its several fields; while, at the same time, our city has not engineers enough to warrant separate organizations. It was wise, therefore, that the founders of our society organized it as they did. I dwell upon this feature of our society because I believe some members do not understand the latitude it permits its proceedings to move in the direction of practical and technical education. Fifty mechanics started the Franklin Institute in Philadelphia, where now the greatest of engineers esteem it an honor and a privilege to be heard, and no city is more distinctly mechanical than Pittsburgh.

It seems to me that the opening of another season of meetings is a good time to consider these things, in order to see what further steps in advance we may make. In the past year it has been sometimes difficult to prevail upon members to furnish papers for discussion, and sometimes I have thought the difficulty was caused by the fear that the subjects chosen might not be interesting. I recall a paper read here once on the subject of whether it was worth while to fire cannon over drowned bodies in the hope of raising them. It was not strictly engineering, neither would the doctors consider it a medical question (doctors never meddle with the dead), so it was brought here, and I believe it was decided that a dredging machine was better than a cannon for the purpose intended, and that the best scoops for dredging machines were made of low carbon steel. Our members appear ready to hear anything, almost. One thing is certain, if the speaker of the evening forgets to give his paper some slight engineering turn, the debaters proceed to infuse that element into it, so that it comes out right in the end. But our members should come to propose something as well as to hear and debate.

During the course of a month there is apt to arise, in almost any member's experience, some problem of a practical nature, of which nothing can be learned from Spon, Mosely, Rankin, Trautwine, Webster, or Worcester, for there are things in engineering too profound for any professor; so, why not ask for information here, as did a member not long ago, who learned how to break cobbles, provided there was any break in them. "In a multitude of counselors there is wisdom," and I believe we meet for the purpose of dealing out that commodity freely. Only one question is barred out, so far as I know, and that is, "How long will the natural gas last?" On natural gas we have several bench marks, and have taken a number of back sights, but our foresights are somewhat hazy, so that our level notes remain unbalanced.

I have thought, considering the diversified talents and tastes of our members, that it might be a good thing to have the society divided into three grand sections, the members being required to join one, with the privilege of joining the others if they desire. The sections in turn might meet on the first, third and fourth Tuesday nights of each month, the second Tuesday night of each month being reserved for the meeting of the general society. This would, I think, be an improvement on the weekly social meetings attempted last winter, because the sections could be organized with a chairman and clerk each, and records could be kept. In this way members might be brought in more intimate contact with others devoted to their special studies or occupations.

I make this suggestion in the hope that members may take it into consideration.

Discussion of Mr. Roberts' paper on "The Future of Our Society."

Mr. Rees: I like that suggestion at the end of the paper, that sections be formed. I think there are some members of the society who would enjoy it better in that way. The civil engineers probably would rally together more numerously and uniformly than they do now if they had a section of their own; possibly the mechanical engineers would do the same.

Mr. Harlow: There were one or two points in Mr. Roberts' address that I was interested in, the first and the last one. That point in regard to dividing the society into several different branches was discussed among the originators of the society, and I remember of Mr. Metcalf remarking at the time that there had been a society formed some time before which was divided in that way, and its death was caused by that action.

It seems to me that considerable interest could be developed by adopting a plan that is used by one of the water works organizations—of the members asking questions. The questions are sent to the secretary, placed in a box, and at the meeting drawn at random from the box, read and answered by some one of the members.

Mr. PHILLIPS: I think Mr. Roberts' suggestion is a very good one, indeed, as to the matter of sections, if it can be carried out. But there is one difficulty that would occur that perhaps is not met in any other of the societies of the same sort. In Philadelphia, for instance, the Franklin Institute, the members, or many of them, are men of leisure. I should think that all of the active members are either men of leisure, or have so much time that it is an easy matter to prepare papers and get ready to discuss them from an actual study of the subject beforehand.

So far as our society is concerned, I think nearly all the members are very busy. Almost everybody is crowded with work, particularly just now, and, indeed, for the last year or two, and that is a drawback that would be encountered in the matter of dividing the society into sections.

Mr. KIRK: I will just say, that while a division into sections seems to be attractive at first sight, I rather think we are not far enough along for that yet; but there is one subject, I think, that would add greatly to the interest of our meetings, and that is to take up the important subject of public highways. A gentleman, who has travelled extensively over the known world, said to me some time ago that he only knew one city in the world that had worse streets than Pittsburgh, and that was the city of Jerusalem.

I am free to say that I do not know any other subject where more power is wasted than in the making of our streets. When I was examining into this subject in Great Britain, I found one day a man busily pinching up the Belgian blocks there in some peculiar way with two little pinch tools. I said to him, "What are you doing here, friend?" "I am bringing this up to grade; it is three-quarters of an inch below grade." In Pittsburgh a man who should make any talk about that would be dubbed a crank at once. Three-quarters of a yard is near enough here. As a result of that I found a very cheap kind of a horse, a horse that would not sell here for more than thirty or forty dollars, was hauling pig metal on the wharf in Liverpool. I saw one load that astonished me at its size. I asked the man how much he had. Said he, "I have only two tons. It was all there were in the pile."

We are making an effort now to have Belgian blocks, but I don't know that they would do much good, for the next week along comes a gas company, and they tear it up and they lay their pipes, and you see them putting the dirt in, and then a man comes along and rams it as if he was ramming eggs and was afraid to break the dirt, and the consequence is, after the street is finished, they have to wheel away four or five hundred cartloads that everybody knows should have been replaced, because you can put in the same dirt into the hole, unless the pipe is very large, that you take out of it, by ramming solidly. However, the result is a rain comes and softens the dirt, which sinks, and then there is a depression in the street, and then along comes a heavy wagon and rams it down, and by and by we wonder what is the matter with the street.

Then, also, there is the employment of prison labor in making these highways. I do not see the sense and propriety of our making a winter residence for a lot of professional idlers. I was told to-day, that, no matter where they are, a lot of these fellows, when the season of cold weather comes, drift into Allegheny county, steal some little thing, and get sent up for ninety days to a fine, desirable country residence, when they might just as well be making roads.

Mr. BRASHEAR: You know I have always taken an interest in the meetings of this society since you did me the honor to elect me a member. I have thought of several plans to improve and interest, but I can hardly agree with Col. Roberts, though I generally do agree with him, in regard to dividing up the society into sections. And I do it for these reasons: In the first place, it leaves me in a bad place. I will have to be a committee of one, if his argument be true. I find I have had very much more interest in the papers read here on different engineering and mechanical subjects than my own particular business, for the reason that I have gotten help, and I can say that I have received good from every paper read here since I have become a member. If a man will watch the little things in life and take advantage of what he reads and sees and hears, he will find how much good he gets from them.

Why, some of the best notions that have been heard of in the handling of heavy machinery have come from illiterate men, Irishmen, who have gone into the mill rough-shod, from, probably, the mast of a ship, who know something of the handling of ropes. If you take advantage of little things you can get some ideas from everybody, if they have any ideas at all.

I think it is a rest for engineers to come here and hear something that is a little outside. But there is another idea. We must teach our members that these meetings are for their enjoyment and mutual benefit in a sort of an outside way from the mere reading of papers. If we have a good paper and publish it we can read it and enjoy it afterwards. It is not for the mere reading of papers that I go to the American Society of Science; you can get those papers in the journals. It is the social intercourse. We have not enough of that here. Why, when we get full here of science, and full of engineering, and full of mechanics, and full of dams, and improvements of rivers, and all that, we forget that we have some of the social ele-

ment in us that wants to come out. Why, it will make us all the better for it.

I don't think we have enough of our social meetings. I would like to come down and shake hands with our good Secretary, and have a little chat with him, or with Mr. Barnes, or Prof. Phillips here, who are shut up in the school room or the rolling-mill all day. We only plod and plod, and after a while they will lay us down in the grave, and we will have had none of the social enjoyment of life that we ought to have had, and it is our own fault; and that is one benefit this society is to us, this of social intercourse. I do not mean to chat about the social affairs of life always, but we could chat about our own particular branch of business, for instance. I never saw two men of intellect meet, but what there were some pearls dropped from one or the other. Why, it will be a help to us in the progress of our own work, and we ought to make this more of a social affair, and afterwards, if we want to divide into sections, all right; I will say amen, like the good old Methodist. But let us get up the social interest. Bring our ladies here once in a while. Our experience on our recent excursion shows what a good time we can have. I worked better for three weeks after that.

Mr. Roberts: I believe I stated all my reasons in the paper for suggesting a division of the society into sections, but I think the members ought to recollect that we have about 320 members, of whom probably 200 are within call within Western Pennsylvania, that is, on the railroads near the city. But our average attendance, while very large as compared with other societies, is only about forty. That leaves us about 160 members within call to attend our meetings. The suggestion was made with the idea of extending the scope of our society, that a member could find friends at the meetings who would be in the same line or profession, the same line of thought, and there would be more apt to come.

I feel like adopting one suggestion made by Mr. Harlow, that we have a question box. I do not know how that is done, but think it would be a good idea to have questions answered by members, and if it was understood by all the members that all questions or technical subjects submitted to the Secretary would be in turn submitted to the Society, it would create a new interest and more members would come to the meetings.

Mr. Harlow: The method is this: The members send their questions to the Secretary, who puts them all in a box, and then reads them at random before the meeting. The idea is to get up a discussion among the members and not to have the Secretary answer them. You do not want someone who will go to some library, but have impromptu answers, where members, whom you can't get to read papers, may be gotten to answer questions. Another point in regard to our papers: When this Society was or-organized, I thought largely the subjects discussed would be outside of my particular interest, that they would be on the manufacture and manipulation of iron and steel, but I have found in watching the papers for several years that, while there were a good many things in the papers I did not care specially about, there was much that interested me, and I think there are but few of our members who have not had the same experience.

Mr. Barnes: I would venture this suggestion, whether more might not be done than is done in the discussion of the progress of engineering works here in Pittsburgh, in which we are all more or less interested, schemes with which some of the gentlemen who come here are certainly familiar, and could give in some informal discussion a description of the work.

I have in mind at this moment three schemes, none of which are of vital importance, perhaps, but concerning which I should like myself some description. One of these is this Baltimore and Ohio railroad station scheme. I go back and forth daily over the Smithfield street bridge, and it is of interest to me. Another is the filtering well that they are going to dig at the old South Side water works, at the foot of Thirtieth street. They are making an expensive excavation there, and I would like to know something about the plan, the cost, and all about it.

One other, which is a little more directly in my line, is the benefits to be derived, and the objections, pro and con, relative to the use of stacks at Laughlins & Co.'s coke ovens for distributing into the atmosphere the smoke from the ovens. I am told that the benefit is so great that the citizens who live on the hill, just above the ovens, pray for Laughlins & Co. every night.

Now, there are other engineering schemes which will present themselves to you gentlemen here, and I think that a description, informal it may be, would be of use to us.

Mr. Brashear then gave the following statement in reference to the pedestal of the

LICK TELESCOPE,

now being erected in California:

Mr. Brashear: Through the kindness of Messrs. Warner & Swasey I was invited to go to Cleveland to inspect the mounting of the great Lick telescope. They expect to have the work completed by the middle of October. It is not only a fine astronomical piece of work, but it is a splendid specimen of engineering work.

The whole telescope will weigh about thirty tons. The tube is being constructed of steel made in Pittsburgh. It will be about fifty feet long, and in the center will be four feet in diameter. At the upper end it will be thirty-six inches and at the lower end about thirty-two inches in diameter. On the lower end of the telescope arrangements are being made, different from any ever constructed before. If I had thought of it, I might have brought the drawings with me. It is so arranged that a sleeve on the lower end of the tube rotates so as to be in perfect control of the observer; to this is attached the hand-wheels by which he can make the necessary movements without taking his eye from the eye piece. He can make every movement necessary; can read every circle, in fact, everything he wants to do, without going away from his comfortable chair or observing stand. This has all been arranged by the manufacturers, who are shrewd Yankees, as well as first-class engineers.

Besides this, there is an arrangement for the assistant, who can control every movement of the telescope without the intervention of the observer himself, so that the observer can go ahead with his observations, simply indicating to his assistant to move so and so. Every desirable movement is brought through the axis of the telescope.

The tube is made in sections, commencing in the center with steel $\frac{3}{16}$ inch thick and reduced to about $\frac{1}{8}$ inch at the ends. The center section is of cast iron, ribbed to insure stiffness. The steel sections are put together on rings of cast iron, which are mounted on a long shaft. These rings form a truncated cone of the correct shape, and when the section is secured on these "spider rings," they are drilled *in situ*, and are then riveted with hot rivets. The sections will be joined together by steel flanges riveted to the ends, and faced off in the lathe. The question of flexure of this tube is a very important factor to deal with. The constructors have calculated it, but whether their calculations and the homogeneity of the steel will come out to the same factor is a question that remains to be actually tried and solved.

Warner & Swasey are now working on the last piece of the pedestal. Every section of it is made, and the whole stand bears some resemblance to the great Corliss engine. The moving parts are so completely balanced that a touch of the finger will move that mass of weight with a beautiful motion only to be seen in these finely balanced astronomical instruments.

It has been a serious question, this of supporting such large telescopes in order to make them move with equal regularity and ease in all parts of the circle. One party, A. A. Common, Ealing, England, has mounted the polar axis of his instrument in mercury, and finds it gives a beautiful motion, but Warner & Swasey have gone back to the old system (and yet they have made it a new system) of mounting the polar axis on steel balls, and so delicate is this mounting that a little touch will cause the instrument to rotate with ease. They have everything beautifully arranged, so nicely, indeed, that a simple touch of the finger, or turn of a wheel, moves everything that ought to be moved, independent of the driving clock, which in itself is a beautiful piece of work.

Mr. Brashear then made a few remarks regarding the proposed measurement of wave lengths of light to obtain a proper standard for all other measurements. He said he had been applied to by Prof. Michaelson (who has measured the wave lengths and velocity of light more accurately than any other man in the world) for some surfaces, which he had described at the last meeting.

The present standard of measure, which dates back to the old barleycorn, or a quadrant of the earth's circumference, is a wrong standard. Instead of a standard that can be burned up or destroyed, Prof. Michaelson proposed to take a wave length of light and make it the basis of our standard. The error in this measurement has heretofore been brought to a minimum of one part in two hundred thousand, but now they have reduced it to one part in five, or, possibly, ten million, so you can get some idea of the nearness to which Prof. Michaelson has arrived. He proposes to take the value which he thus obtains, and rule it with a diamond point right on a bar, so we can take the bar and use it as a standard of measurement, and then, no matter whether that standard bar be destroyed or not, we have the wave lengths as a constant measure, which are an invariable standard, unless, perchance, there should be some change in the density of the ether in space, a consideration hardly worth discussion.

From the foregoing, added Mr. Brashear, you can see how near we are approaching the border line of the infinite.

On motion, society adjourned.

S. M. Wickersham,
Secretary.

ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

Society met November 15th, 1887, at 8 o clock, P. M. Present, forty eight members and six visitors. President Dempster in the chair, Vice President Brashear assisting. Messrs. A. H. Enroer, of Allegheny; A. P. Kirkland, of W. P. R. R.; Thos. Dobson, of W. P. R. R., and F. M. Hopke, of Linden Steel Co., were duly elected members.

The discussion of Mr. Ramsey's paper was continued, viz.:

MR. BUCHANAN—Mr. Ramsey has prepared his paper in a very careful manner; in such a manner that it is pretty hard to question his statements. He has said nothing, however, that is particularly new to engineers who have been looking into the matter for several years. The greater part of the experiments he outlines were made at the instance of the engineers of the St. Louis bridge, during the building of that structure, and I understand no alarming results were obtained. Some engineers have embodied the temperature tests in their specifications for six or seven years, to my recollection, and I do not know how much longer. (See Table, page 166.)

In the twenty tests here tabulated an effort was made to break the specimens as rapidly as could be done without impact, to ascertain whether a shortening of the duration of test would alter the results obtained by Mr. Ramsey. The only variation observed was that specimen which was still at a low temperature at point of fracture when it failed. The appearance of fractures, strength and ductility showed no perceptible variation from results obtained in every-day practice, except that the strength per square inch was somewhat greater in the specimens broken at a low temperature.

The bars nicked and broken transversely under the hammer gave a variety of fractures, as noted in the table, but in no case did the fracture occur suddenly, or with indications of brittleness

In preparing specimens, two pieces were cut from one end of a bar and marked 1A and 1B, 2A and 2B, &c. From one side of each of these specimens a strip was cut for tension test, and the remaining part used in the transverse tests, so the four tests having the same number are all from the same bar of iron.

Some transverse impact tests lately made of iron carefully manufactured from specially selected stock, gave uniformly good fractures, both at low and normal temperatures, and duplicates have been laid aside for further tests at a time when the temperature may be lowered naturally.

MR. ROBERTS—It is surprising, at this late period, we might almost say the period which marks the end of the iron and the inauguration of the steel age, that there should remain any doubts as to whether freezing weather makes iron more brittle than it is at a normal temperature; yet the statements of Messrs. Ramsey and Hunt, at our last meeting, tend decidedly to disturb the general conviction, which is opposed to their view.

When I say the general conviction, I refer to the opinions of metallurgical experts and their followers among engineers and manufacturers. As for popular opinion, I believe it is, on this subject, voiced exactly by Mr. Ramsey's axeman, who warms his axe on a frosty day; although on this score I have heard lumbermen say that they broke more axes in winter because the wood was full of frost. In Clarion county, Pa., one winter, when the thermometer was twenty degrees below zero, I have seen hickory saplings broken short off by a sudden blow.

I think I am correct, also, in saying that even engineers, a quarter of a century ago, partook of the popular belief that iron is more brittle in cold than in warm weather. We sometimes change our minds in regard to matters of fact so gradually, by observation and experience, that we cannot, when called upon in later years, recall all the reasons which brought about the change of views. This is especially the case, I have no doubt, in facts of this nature when information is not obtained by the individual from original research.

I regard the question as one of the greatest importance, and I believe our Society in this great center of iron industry is in duty bound to set it at rest. If I mistake not, there have been other series of experiments, possibly some by the United States Government Commission, which lead to conclusions the reverse of those reached by Mr. Ramsey, and I shall hold my opinion in abeyance until I have time to look them up, or until Messrs. Ramsey and Hunt flood the field with unmistakable evidence of the correctness of their position. They should be allowed ample time and be

properly supported in the experiments which they will, I hope, conduct.*

In one of Mr. Ramsey's minor conclusions, viz., that cold increases the tensile strength of iron, which belief, he says, was "supported by the tests for tensile strength in the testing machine," I can refer him to experiments made as long ago as 1860, which lead to an opposite belief. I quote now from the Annual of Scientific Discovery for 1863 (experiments by Mr. Kirkaldy), page 157:

"A bar of Glasgow best bar iron, of three-fourths of an inch diameter, was forged into ten bolts, and six of them were exposed all night to intense frost in the month of December, 1860, then tested the next morning, when the thermometer stood at twenty-three degrees Fah. The other four bolts were kept warm all night and protected during testing. Three of the bolts were tested with gradual, and seven of them with sudden strains. With gradual strains the bolts exposed to frost gave way with 54.385 pounds' strain. The unfrozen bolts stood a strain of 55.717 pounds. A difference of 2.3 per cent in favor of the latter. When submitted to sudden strains the difference was 3.6 per cent. in favor of the unfrozen bolts."

Mr. Kirkaldy's experiments were not numerous, it is true, but care seems to have been exercised to have the samples from the same bar. I would understand that the forging spoken of had been done before the bar was cut. The experiments appear to be inconclusive, just as they ought to be according to my present view, but show enough to raise a conflict with Mr. Ramsey. If the tensile strength of iron is increased by cold there must be a rule found for it.† This is in line with a portion of Mr. Ramsey's theories, or rather, I should say, deductions, for Mr. Ramsey has not presented himself as a theorist on this question, rather, on the contrary, dealing with uncomfortable statements—"frozen facts" he doubtless considers them. So, at present, I have nothing to say in regard to his experiments, except to admit that his specimens of ruptured bars have fully as ugly an appearance as

*I can refer now on this point only to a statement in Engineering, February 13th, 188 , summarizing the result of Mr. Webster's experiments in England, as follows:

"When bars of wrought iron or steel, malleable cast-iron and common cast-iron were subjected to impact at a temperature of five degrees Fahr., the force required to break them and the extent of their flexibility were reduced as follows:

	Reduction of Force Impact. Per Cent.	Reduction of Flexibility. Per Cent.
Wrought Iron,	3	18
Best Cast-steel,	3½	17
Malleable Cast-iron,	4½	15
Cast-iron,	21	Not taken.

The behavior of the cast-iron seems remarkable. I can see no reason for it becoming relatively more brittle than the others. It is to be admitted that the statement is worthy of serious consideration. T. P. R.

†According to experiments conducted by Mr. Kallmann during 1877 and 1878, at the Oberhauser Works, there was no difference in the tensile strength in the case of either fibrous, fine grained iron or steel between the freezing and boiling point of water. Above 600 degrees temperature there was a marked falling off in their strength. (See Engineering, July 30th, 1880.) It is proper to note that the experiments were not tried quite so low as the freezing point. The greatest strength, viz., 100, was assumed at that point as the base of the scale, and it was still 100 at 212 degrees for all of them. T. P. R.

REPORT OF TESTS OF BRIDGE IRON, MADE AT THE UNION IRON MILLS OF CARNEGIE BROS. & CO. LIMITED, PITTSBURGH, PA.

Fracture under Nick.		Marks.	Section.		Area	Elongation.		Elastic Limit.		Breaking Strain.		Date of Test.	Reduction.			Temp. ° F.	Carbon.	Manganese.	Phosphorus.	Sulphur.	Silica or Silicon.		Duration of Test.	Remarks.
At 25°	At 80°		Iron.	Specimen.		In 8ins.	Per Ct.	Specimen.	Pnr Sq. in.	Specimen.	Per Sq. In.		Dimensions.	Area.	Per Ct.									Emory Machine.
55% crys.	....	1a	6"× ⅞"	.750	.4418	1 52"	19.	14,300	32,370	24,300	55,000	May 12,'87.	.653	.3349	24.2	32°	.08	.07	.289	.012	.60	.280	40 sec.	Clean fibrous fracture.
....	Fibrous.	1b	" "	.802	.5052	1.75"	21.9	15,800	31,280	26,400	52,260	"	.703	.3881	23.2	80°	.08	.07	.314	.007	.54	.251	1m. 41s.	" " "
7% crys.	....	2a	6"× 1"	.806	.5102	1.45"	18.1	......	......	......	......	"	.720	.4071	20.2	32°	....	....	....	....	....	....	5 sec.	" " "
....	Trace of crys.	2b	" "	.809	.5140	.93"	11.6	......	......	......	......	"	.718	.4049	21.2	80°	....	....	....	....	....	....	9 sec.	" " "
15% crys.	....	3a	" "	.812	.5178	1.47"	18.4	16,700	32,255	26,400	50,990	"	.717	.4038	22.	32°	....	....	....	....	....	....	1m. 58s.	Fibrous, with trace of crystal.
....	3% crystalline.	3b	" "	.783	.4815	1.48"	18.5	15,800	32,820	24,20[illegible]	50,260	"	.687	.3707	23.	80°	....	....	....	....	....	....	32 sec.	Clean fibrous fracture.
....	....	4a	6"×1¼"	.808	.5128	1.32"	16.5	......	......	......	......	"	.719	.4060	20.8	32°	....	....	....	....	....	....	4 sec.	" " "
....	Trace of crys.	4b	" "	.803	.5064	1.40"	17.5	......	......	......	......	"	.715	.4015	20.7	80°	.08	.08	.279	.043	.55	.256	7 sec.	" " "
95% crys.	....	5a	" "	.810	.5153	1.88"	23.5	15,500	30,080	26,700	51,820	"	.689	.3728	27.65	32°	.08	.08	.308	.005	.62	.289	35 sec.	" " "
....	....	5b	" "	.811	.5166	2.02"	25.25	14,900	28,850	26,000	50,330	"	.708	.3937	23 8	80°	....	....	....	....	....	....	55 sec.	" " "
Fibrous.	....	6a	5"× ⅞"	.774	.4705	1.75"	21.9	......	......	......	......	"	.644	.3257	30.8	32°	.09	.07	.154	.007	.46	.214	11 sec.	" " "
....	Fibrous.	6b	" "	.786	.4852	1.98"	24.75	......	......	......	......	"	.629	.3107	36.	80°	.09	.08	.153	.032	.47	.219	8½ sec.	" " "
25% crys.	....	7a	4"×13-16"	.819×.805	.6593	1.63"	20.4	18,400	27,910	34,100	51,720	"	.664×.716	.4754	27.9	32°	....	....	....	....	....	....	1m. 15s.	" " "
....	Trace of crys.	7b	" "	.822×.807	.6634	1.89"	23.6	15,700	23,670	33,700	50,800	"	.665×.749	.4781	27.9	80°	....	....	....	....	....	....	1m. 21s.	" " "
10% crys.	....	8a	4"× ¾"	.745×.730	.5438	1.40"	17.5	......	......	......	......	"	.620×.644	.3993	26.5	32°	....	....	....	....	....	....	12 sec.	" " "
....	Trace of crys.	8b	" "	.749×.735	.5505	1.34"	16.75	......	......	......	......	"	.627×.647	.4057	26.3	80°	....	....	....	....	....	....	8½ sec.	Trace of crystal, and flaw in we
30% crys.	....	9a	4"×13-16"	.816×.811	.6618	1.52"	19.	......	......	......	......	"	.658×.708	.4659	29.6	32°	....	....	....	....	....	....	11 sec.	Clean fibrous fracture.
....	4% crystalline.	9b	" "	.815×.810	.6601	1.77"	22.1	......	......	......	......	"	.672×.722	.4852	26.5	80°	....	....	....	....	....	....	11½ s.	" " "slight flaw.
5% granular.	....	10a	5"× 1"	.803	.5064	1.74"	21.75	15,300	30,215	25,500	50,360	"	.653	.3349	33.9	32°	....	....	....	....	....	....	1m. 5s.	" " " light and dark
....	Fibrous.	10b	" "	.809	.514[illegible]	1.72"	21.5	16,000	31,139	24,9[illegible]0	48,450	"	.671	.3536	31.2	80°	....	....	....	....	....	....	1m. 12s.	" " "

hey are stated by him to have. We must "explain them away," if it takes all winter, otherwise I fear Mr. Lindenthal's next bridge will be a hemp rope suspension. But I think Mr. Ramsey is vulnerable in his theories, and it will be some satisfaction to catch him up on some of them, even if his points are established. Thus he says, "Iron at seventy degrees is hard; reduce the temperature 100, or to minus thirty degrees, and why should it not become harder and more rigid? As the fluid iron has changed into a solid fibrous material by reducing the temperature, why should not a further reduction of temperature change the fibrous to a crystalline structure?"

I have no doubt that iron becomes harder by the reduction of temperature. It contracts with cold, and suppose we could create a degree of cold sufficient to cause a cubic foot of it to shrink to a half a cubic foot volume; it ought to be then twice as hard, because its density has been doubled. But to say that such a surprising result, as a change from fibrous to granular, can take place in the organic structure of wrought iron in so short a distance of its temperature scale, as Mr. Ramsey speaks of, is beyond my conception. The fuzing point of iron is 2,900 degrees F.; that, in a sense, corresponds to the melting point of ice, and great and wonderful are the changes which take place at the point where any solid melts Above that they are liquids until they reach the point of sublimation. Iron, it is true, becomes incandescent before it reaches the melting point. We may take bars of pure iron in any part of the scale below the welding point down to freezing of water (or far below, for that matter, I think), and find the structure fibrous in all cases. Heated to redness and plunged into cold water, making a sudden change of 2,000 or more degrees, does not effect any perceptible change in the molecular arrangement of the particle of which it is composed. Why, then, should a change of 100 degrees, or, as Mr. Ramsey's tables in some places show, of only forty degrees, somewhat farther down in the temperature scale, develop such a metamorphosis? Unfortunately we cannot look into the inside of a bar of iron to satisfy ourselves in regard to these questions, but I can see no reason for fibrous iron becoming granular in the particular part of its temperature scale corresponding to the freezing point of water. I do not perceive the analogy between the substances, iron and water, though, possibly, there is some mysterious arrangement in which water, or its vapor, plays an important part, if such changes actually occur.

So far as I know, the contraction of iron proceeds by the ascertained rule past the freezing point of water without exhibiting any peculiarity of movement indicating that crystallization was taking place at any particular moment. Water, in freezing, differs from all other liquids in ceasing to contract several degrees (five degrees) above the point at which it becomes solid. At the moment it freezes it suddenly expands one-twelfth in volume, reaching the volume it previously occupied at a temperature of forty-eight degrees. This remarkable and sudden expansion develops a force which is competent to break up or disintegrate any material which opposes itself to its power.

Referring to crystallization, generally, Stockhardt, in his Principles of Chemistry, says: "Thus, in inanimate nature, a mysterious power exists, similar to that which compels bees to construct their six-cornered cells, and the potato to produce its five-angled corolla, and five stamens, and by which the smallest particles of bodies, called atoms, are forced to arrange themselves in a fixed order assuming a regular shape. *But this can only be accomplished by a body in its fluid or ariform state,* since a free motion of the atoms is essential." He says crystals may be formed in only three ways. 1*st*. From a solution, either by cooling or by evaporation, as saltpetre, for instance. 2*d*. From a molten fluid by congelation, as in the case of sulphur. 3*d*. From vapor when it becomes solid, as snow and iodine. If it can now be shown that cold, solid, fibrous iron becomes crystalline in structure during its passage past the freezing point of water, our ideas of the rules governing crystallization are revolutionized.

The fibre in wrought iron is, no doubt, imparted to it in a manner somewhat analogous to that given to taffy, which receives its "grain" from "pulling." The iron is worked in the puddling furnace until all, or nearly all, the carbon left from the pig metal is burnt out. It is stirred through and through many times by the puddler's bar, and emerges from the furnace a striated plastic mass. The after piling, rolling, etc., may add more fibres and lengthen those given it by the puddler.

Often, in bars which have been broken, we can see flakes of cinder, which have been lengthened and widened by rolling, separating layers of fibrous iron. It occurs to me that foreign substances, such as this, would be greatly disturbed by crystallization, and the appearance of such foreign substance in iron might furnish an index to any molecular disturbance which changes of temperature might have produced, particularly if the operation was alternately repeated a number of times.

There is another point to which I ask attention. Judging from some samples of bars I recently had broken, there is iron sold in this city as "the best Juniata" which is very irregular in texture in the same bar. It may be equally strong in different parts, as regards tensile strength, but it is certainly unequal as regards strength to resist sudden impact in different parts of the same bar. Thus, a sample of a bar 1½

inches in diameter, which I exhibit, shows at one end about half crystalline and half fibrous grain, while only two inches away it is about 90 per cent. crystalline and 10 per cent. fibrous. The different rods of the same brand appeared to be equally irregular in this respect. I can account for this only on the theory that the original balls from which they were made were either not worked enough, or worked too much in one direction. The puddler's "striations," we might say, were vertical, and it was rolled, or first flattened, or so as to separate the fibres and lengthen the areas of granular structure. Several similar rods of another brand of wrought iron, a specimen of which I present, showing less than 10 per cent. of granular structure, were broken in handling by being dropped on a wall; this, too, during warm weather. Unfortunately, I could not find the rods which had been thus broken, but, no doubt, they were highly crystalline at the points of rupture. I am tempted to believe that with the improvement in steel manufacture the iron men are growing careless in their puddling. At all events, I think great caution should be exercised in the choice of bars for future experiments which are to go on record in this discussion.

Mr. Reese: When you talk about crystalline and fibrous structures, that is a different thing entirely. The fibre in iron, my friends, is caused in this way. There is no fibre in iron *per se*. There is more fibre in puddled iron than in any other form. I find that it is more uniformly fibrous. The reason of that is that the silicon comes out first and then the carbon comes out and the iron assumes a condition like flakes of snow floating from the clouds above. They go floating around in the furnace, one or two at the start, then in myriads until they get so thick there that they will interlock each other until they form that mass which is like a sponge, and we turn them over so that all sides may obtain equal working, so as to oxydize the carbon and bring the iron to nature. When we get that all right we have to take care to form it in four or five balls, for if not it would stick so as to be difficult to get in proper shape. We keep turning these over and over; we oxydize a part of the iron, the oxide runs over and coats it, and then we squeeze these particles all together and the interstices become filled with this oxide. Now, when they get to rolling that out into the bar they draw the iron out. We find there is a streak of irön and then a streak of oxide that covers it and then another piece of iron, and the bar is in reality the oxide and the iron welded together. When the iron is well oxydized it will show good fibre, and when the oxide is absent the fracture will be granular.

Mr. Koch: I will be very happy to continue this discussion, but it compels me to go back many years, for it is a long time since I had anything to do with iron, probably fifteen years. It was in the early days of steel and we were suffering terribly from the attacks of the iron men, and there was considerable discussion on the structure of iron and steel.

I remember one experiment that may throw a little light on this question. You know the climate of England is peculiar. In the winter season, during a rain storm, it is as likely as not to turn cold suddenly so that you will not have time to get your umbrella down before it is frozen stiff. When that happens it plays the very mischief with iron structures, especially if unpainted. We made some very curious experiments. We got some good fibrous iron bars unpainted, and put them in salt water; then we took them out and gave them a good washing, and then left them in fresh water, and then we froze them, and finally on investigation it was found that the iron by this process had lost its roundness. And what made it happen? Iron being fibrous, and there being room between the fibres, not being a homogeneous material, like steel, the water penetrated between the fibres, and being frozen there it had the effect of changing the condition of the bar so far as its shape was concerned. We put the bar in the microscope, and after being treated in this way we could see the interstices.

To make the experiment more exact, we took hyposulphite of soda and treated the iron with this solution. The iron was left there for about six or seven months and then it was taken out, sliced and put in the polariscope, and we were astonished to see the effect produced by this treatment. You would be astonished to see how perfectly iron can absorb materials in solutions of this kind. This can be remedied to a large extent, however, in the case of structural materials, as well as others, by keeping them well painted.

In the matter of steel. We did not make as good steel fifteen years ago as we do now. We were very unhappy to find that we got some very open crystalline structures in steel. I have tried it since, however, and am glad to say we have improved so far that I do not get any such open crystalline structures. But I think the occurrence of crystalline and fibrous structures in the same piece is not so uncommon as people imagine. You can see the same thing in ice. I remember Prof. Tyndall showing me on one occasion a piece of ice that had a crystalline structure in one place and a fibrous in another. When I asked him how he accounted for it, he replied that he could not account for it.

However, the result of our experiments in the absorption of water by iron and its freezing may account for some of the things which we are now unable to explain. What I tell you is very old, for when I talk about iron it is to me like going to a Pullman car of-

fice and asking for a seat in a stage coach to Philadelphia.

Prof. Phillips: It seems to me in this connection it might be said that, *a priori*, we could conclude that changes of temperature would have some such effect as stated in iron. There are so many cases well known where slight changes of temperature have produced great changes in the physical qualities of bodies, and it might almost be considered to be the exception to find this not to be the case. I may mention a few of these. We might cite, for instance, arsenic. Metallic arsenic condenses in two perfectly distinct forms, one a soft black powder; the other is a brittle crystalline substance, intensely hard and difficult to break in a mortar; whether arsenic assumes one form or the other, depends wholly on the temperature. The metal antimony is another. I have two beautiful specimens, one silver white, not at all like the commercial antimony, and the other is a dark grayish brown metal. These are the natural surfaces, and are very brilliantly lustrous. They are quite different, especially in the matter of brittleness.

The same thing is true of carbon, as everybody knows. By heating, the diamond is converted into a substance like coke. Heat causes the most wonderful change in the hardness and brittleness of metals. The metal zinc is subject to such changes as these; zinc can only be rolled at a heat of 100 over the boiling point of water.

Mr. Brashear: I wish to make a remark in relation to one point Prof. Phillips spoke of, which was also brought out in the remarks of Mr. Reese. Prof. Phillips remarked that there were certain anomalies in cryatalline structures, but notwithstanding this there was a correlation existing, that, if understood, would help us to unravel some of the mysteries surrounding the structure of iron and steel. Mr. Reese remarked that it would be a good thing if we could see inside of iron. I have made a great many studies of iron and steel in years that are past, and from my knowledge gained in the new pursuit that you know I now follow, I am confident that many of the characteristics of glass have a close correlation in iron, steel, or perhaps any crystalline structure. It would be an unfortunate thing for the optician if he did not know the internal condition of the piece of glass that he wanted to work into a lens. It may seem curious to many persons not versed in optical science, but it is an absolute necessity that the optician should be able to tell upon an examination whether the atoms or molecules of his glass were in a normal condition or not. Suppose, for instance, that my glass is perfectly clear, that it has the proper specific gravity, that it is free from striæ and cords, and suppose that I know all these facts, am I now to say that the glass is in condition to make into a lens? Certainly not. I must know that my glass is well annealed, that is, that the glass has been so slowly cooled that the molecules have been allowed abundant time to fall into such a condition or position in which one shall not be pulling abnormally upon another. After all, this is the simple explanation of annealing, either in glass or metal. Fortunately, we have a means by which we can at once tell what the condition of our glass is. By passing a beam of polarized light through it, the effect upon this beam as it comes into the human eye is such that its conditions are not altered if the annealing is perfect, but if, in the structure of the glass, there are places where a strain has occurred, it will deflect this beam of polarized light, and as a consequence produce changes that are instantly recognized even by the unpractised eye. A glass manufacturer once said to me, "I wish I could tell when my glass is perfectly annealed." I told him that there was no difficulty to tell annealed from unannealed glass. He seemed to doubt my word, but upon my showing him the strains in glassware which he had made himself, and proving to him that they were strains, he was about the most surprised man I have ever seen He remarked that this would save him thousands of dollars a year in his cut and engraved glass, as many times he found his glassware broken to pieces after the engraving had been done, and much expense put upon the ware. I presume glass manufacturers would not care so much for a polariscope for use in the ordinary glassware they send out, but we know glass will break anyhow, annealed or unannealed. I was one time working a disc of glass weighing twenty-five pounds. There was a little projection standing up on it which, to save time, I took a hammer and knocked off. Instantly the disc flew into many thousand pieces, nearly all the breaks occurring diametrically. Upon examining with the polariscope it was seen to be full of circumferential strains, which caused the breakage, only needing a start, as in knocking off the little chip I have made many experiments since in this direction, and can tell with almost absolute certainty how a piece of glass will break. When my friend Alvan Clark received the glass for the photographic correcting lens for the Lick telescope, he examined it with a polariscope, and told me that he was almost certain that it was going to break in the process of working it. He wrote to the maker and told him that he would work it only under protest. The maker took the responsibility. Mr. Clark commenced the work on the glass but had proceeded only a short distance when the disc flew into many hundred pieces.

Now, I sincerely believe that these conditions hold good in iron and steel. I believe if iron is rapidly cooled either as a casting, finished iron, or steel, that

the molecules do not have time to fall into a normal condition, and that ever afterwards there is a constant strain and pull within the material itself which tends to weaken it more or less. In the hurry to make iron and steel we have often seen the muck bars cooled as rapidly as possible so as to cut it up, and pile it so as to put into the furnace again to make the finished bar. This bar is put on cooling beds to cool it as rapidly as possible, and it is sent out to the market in this condition. In our hurry for a casting, it is often taken from the sand red hot, and is cooled off with water so as to use it as quickly as possible.

Is it not morally certain that the same conditions will hold in metals treated in this manner as we can actually see in our too rapidly cooled glass? I believe there is a correlation, as I said in the first, between all crystalline structures and that which we can see in glass, which we may be quite certain exists in iron and steel when the molecules have not been allowed to fall into that position designed by old mother Nature.

Mr. G. W. G. Ferris: I would like to say that some time during the winter of 1883 I had the pleasure of making some experiments for the Lake Erie Iron Company, of Cleveland, Ohio. These experiments were made on iron railroad car axles about four inches in diameter, and which were taken from a pile of some two hundred and fifty or more, which had accumulated during a month or six weeks work, and which were piled in the open air subject to the cold lake winds. The day these experiments were made the temperature was 2° delow zero. From this pile of two hundred and fifty axles I selected five which were so cold that they could scarcely be touched with the fingers; in other words, the iron was as nearly in a frozen condition as would probably ever occur. I had these axles taken from the pile to the place where they were tested, a distance of perhaps sixty feet, and in the open air. The tests which I made on these axles were drop tests, and as nearly as I can recollect as follows, the axles being numbered 1, 2, 3, 4 and 5:

No. 1 was placed upon a cast-iron frame with points of support thirty-two inches apart. The weight, which was 1640 lbs., was then raised ten feet and allowed to fall upon the axle, the deflection produced being three and one-half inches. The axle was then reversed so that the convex side was upward and the weight raised ten feet and allowed to drop on the axle again. This second fall of the weight not only completely straightened the axle, but produced a deflection below a horizontal line through the centre of the axle of one-eighth of an inch.

The axle was again reversed with convex curvature on the upper side and the weight again raised to a height of ten feet and allowed to fall upon the axle. This blow produced a deflection of four and one-eighth inches below a horizontal line through the centre of the axle. Upon examination of the axle at this time, it was found that the metal adjacent to the point which had been subjected to the impact blows had become considerably warmer, so that in fact the bare hand could be placed upon the iron at this point without discomfort. I then allowed the axle to remain as it was during a time of about forty minutes, when it had again, apparently, become of uniform temperature throughout.

The axle was then reversed with the convex side upward and the weight raised to a height of fifteen feet and allowed to drop. This blow not only straightened the axle but produced a deflection below the centre line of one and one-fourth inches. The axle was immediately reversed, convex side upward, weight again raised to a height of fifteen feet and allowed to fall. This blow straightened the axle and produced a deflection of four and one-fourth inches below the center line. Upon examination of the axle at this time the metal adjacent to the point where the weight was applied was found to have become very much warmer than the ends of the axle, but there could be discovered no rupture of the external fibres or separation of fibres in a longitudinal direction, showing that so far as outward appearances were concerned, the welding of the metal still remained intact. The axle was again reversed, convex side upward, and the weight was run up a distance of twenty feet and allowed to fall on the axle, the result being that the axle was entirely straightened by the blow and that fracture took place in a vertical plane perpendicular to axis of axle, of almost every fibre in the entire area. The examination of this fracture showed that every fibre or bundle of fibres throughout the entire section had been broken as by pulling apart in tension; there was absolutely no granulation, or so-called crystallization, to be seen in the broken section.

The next two axles, Nos. 2 and 3, were experimented upon in the same manner as No. 1, with substantially the same results.

No. 4 was subjected to two blows from the same weight raised twenty feet, and broke with the second blow, and showed no sign whatever of granulation; but there was for the depth of about seven-eighths of an inch, on that portion of the fracture near the concave side of the curve, an appearance similar to the so-called crystalline condition. Upon careful examination of this part of the fracture it could be seen that this appearance was due to the fact that numbers of separate fibres, which were in close contact, and which had appeared to bundle themselves together, had been sheared by the effect of the blow. This was evident from the fact that these little bundles of fibres

could be seen when looking in a direction parallel to the length of the axle, and also that the ruptured end of any bundle of fibres showed a plane and mirror-like surface.

No. 5 was broken by the first blow of the weight, which fell from a height of twenty-five feet, and the fracture showed a similar condition of fibre to that existent in No. 4.

The very fractures found in Nos. 4 and 5 are generally taken to be a crystal'ine state of the iron. This is, in almost every case of fracture produced by transverse stress, not crystallization at all, but simply the sheared surfaces of a small number of fibres which have been freed from the welding slag, generally by vibration, bundle themselves together, and upon rupture show a sheared surface which may be either plane, convex or concave. Upon thousands of examinations of fractures in wrought iron, produced by either tension, bending or nicking, I have never been able to discover anything whatever that would take any definite form, as would be necessary in a state of crystallization. I have, however, seen in cast iron something that certainly seemed to take definite form and would appear to approximate to the crystalline state.

The conclusions to be drawn from these experiments are simply that the subjecting of wrought iron to freezing temperatures does not necessarily produce in the iron a state of brittleness, and I am not willing, as yet, to admit that any such condition as crystallization in wrought iron can exist.

I have been very much pleased with the remarks made by Mr. Brashear, as he commences at the proper point to obtain satisfactory information on this subject.

When the demand for muck iron is so great as at the present time, it simply means that an inferior muck iron will, to a large extent, be used. I have seen conditions of things in mills like this: pig iron from the same blast furnace, having been puddled in two different furnaces, would in one case make a first class muck iron, and in the other muck iron which was unfit for use. Too much care cannot be taken in the puddling of iron and working of same into muck bar.

In this same connection it may be said, that of twenty-five bars of iron rolled by the same man, the iron being puddled in different furnaces, but coming out of the same heating furnace, and rolled into finished material, you can find a difference in the quality of every bar of the iron. All this condition of uncertainty is not so noticeable or so liable to occur in soft steel. I have in my long experience in this sort of work, and from the vast number of experiments which I have made, come to the conclusion that we are rapidly approaching the soft steel age. The reason for this can be found in the fact that, generally speaking, the manufacturers of steel for structural purposes are using much greater care in their methods than are the manufacturers of structural iron. The latter are allowing their product to decrease in its uniformity as well as its quality, while the former are making an average product daily.

Mr. Munroe: In regard to temperature, I think it has a great deal to do with iron. In working it in boilers, my experience in years gone by was that we at all times in winter had to heat up our flues, for if we did not the result would be cracks. After we got in the shop and had the same temperature we had no cracks. The iron was good in every respect—charcoal iron. Now we do not see as much iron as formerly Some time ago a steel head was brought around to be flanged. It was about five-eighths inches in thickness, to be flanged to forty inches diameter. It flanged beautifully, just as nice a head as I ever saw. We stood it to one side. It was probably two or three o'clock when it was flanged. The next morning before dinner time it went off like a cannon, splitting across the flange down almost to the lower edge of the lower flange. Nothing was touching it at the time and we thought it very strange. On analyzing it, it was found to contain .56 % of carbon. It was very granulated. I just mention this, showing that Bessemer may work very well under heat and yet be very short in the grain. Because it was cooled off suddenly there was a very great strain there. We now anneal all our heads and have no trouble in their breaking.

Mr. C. P. Buchanan, Jr., then read the following paper on

SOME TESTS OF STEEL BEAMS.

Mr. President and Gentlemen:

During the early part of the present year the management of the Pennsylvania Lines, Southwest System, had occasion to use some girder bridges at points where circumstances made it necessary to build the girders of minimum depth. Carnegie, Phipps & Co. Limited, had just announced their readiness to furnish twenty (20) inch steel beams, and it was decided that such beams would satisfy the requirements.

The manufacture of these beams was, however, rather a new departure, and I was instructed to make such tests as would furnish abundant proof of their fitness. As I have heard of no other tests of such large beams, and as the results proved interesting to myself, I thought an account of the making of the tests, with data, might prove of interest to some of our other members, and that a discussion of the methods used and results obtained would certainly be of benefit to all of us that are interested in work of this kind.

The first beams rolled were of open-hearth steel, and for making test, a beam was used sufficiently long

to permit of the placing of supports twelve (12) feet apart, center to center. The backing for supports consisted of four (4) fifteen inch iron beams, weighing fifty (50) lbs. per lineal foot, resting against the cast blocks of the testing machine. The weight of beam tested, estimated from cross-section, was 72.62 lbs. per lineal foot. Moment of inertia was 1343, and of resistance, 134.3. Loads were applied at center of span, and deflections measured at same point, as shown in sketch.

It may be well to say here that the manner of measuring deflections was objected to, but, without giving the matter much thought, was used upon the assurance that it was all right, for it had been used before in making tests of short girders.

The following table shows the loads applied and the measured deflections:

Load, Lbs.	D. in Ins.	Deflections.		Lat. Deflec. at Center.	
		For Inc.	Total.	R.	L.
17,190	1.6687			0.	0.
34,380	1.5974	.0713		.02	
51,570	1.5267	.070	.1420	.03	
68,760	1.4627	.0640	.2060	.04	
85,950	1.4058	.0659	.2629	.05	
17,190	1.6512		.0175	0.	0.
85,950	1.4480	.2207			
103,140	1.3937	.0543	.2750	.06	
120,330	1.3310	.0627	.3377	.06	
17,190	1.6582		.0105	0.	0.
120,330	1.3332	.3355			
137,520	1.2626	.0706	.4061	.07	
154,710	1.1832	.4855			
171,900					
17,190	1.5660		.1027		
195,000					
0	Compression Flange,		1.02	2.60	
0	Tension	"	.87	.65	

When the load was increased to 195,000 lbs., equivalent to 52,270 lbs. stress on extreme fibre, the web, buckled above center of beam and under point of application of load, and top of flange of beam, was crippled before the pressure could be released. The stress on extreme fibre at the elastic limit was 41,470 lbs.; so the test to this point was considered satisfactory. But when the deflections gave 13,742,000 as the co-efficient of elasticity there seemed to be something wrong

Pieces were then cut from uninjured portions of the web and flange and tested in the Emory machine at the Union Iron Mills of Carnegie Brothers & Co. Limited.

The specimen from web was tested in tension and showed permanent set at 42,860 lbs. per square inch, and a maximum strength of 76,905 lbs. per square inch; elongation, 18.25 per cent., and reduction of area 48 per cent., with a co-efficient of elasticity of 29,995,000 lbs.

The specimen from flange was tested transversely, the supports being ten (10) inches apart, center to center, and loads applied midway between. This showed increased deflections at 67,570 lbs. stress on extreme fibre, and sustained a maximum load equivalent to 117,300 lbs. stress on extreme fibre, with a total permanent deflection of 1.1 inches. The co-efficient of elasticity deduced from the deflections within the elastic limit was 30,430,000. Merely as a matter of curiosity this specimen was reversed, straightened and tested in tension in an Olsen screw machine, giving 36,510 lbs. per square inch as the limit of elasticity, 78,230 lbs. per square inch maximum load, 18.5 per cent. elongation, and 34.1 per cent. reduction.

These results set me to work to ascertain whether figures would indicate any deflection in the supporting beams used in the first test; and assuming the co-efficient of elasticity of said beams to be 25,000,000 lbs. (tests of similar material had shown this to be about right), I found a very considerable deflection, which reduced the measured deflections of the steel beam so much that the co-efficient of elasticity for that beam now appeared to be 19,807,000 lbs. Two of our members said that was about right; that the co-efficient of elasticity was always much lower in large sections, and the larger the section, the lower the co-efficient. Another member said he had demonstrated by a series of tests that the fixed numeral in the formula

$$\frac{P}{48d} \frac{l^3}{I}$$

should be 32. He had the indorsement of one of our members, who is a leading light among bridge engineers, and the substitution would have made the result in this test about 29,700,000 lbs., but I was not satisfied that either theory was right. Finally, another of our members said that perhaps the short span and consequent rigid nature of the girder might have caused an excessive deflection under the load, instead of bending the beam to the elastic curve, and suggested making another test with the load applied at two points.

Arrangements were accordingly made to make a test in that manner. The loads were so placed as to produce the maximum bending moment in the beam, and an apparatus was designed to permit the operator to measure the actual deflection. All of which is shown in sketch number two (2).

The following table shows the results in detail. The beam used was of Bessemer steel, weighing 66.23 lbs per lineal foot, calculated from cross-section. Moment of inertia was 1147, and of resistance, 115.

LOADS. LBS. TOTAL.	Deflection.				Lateral Deflection.			
	for Inc.	Centre.	A	B	Cen. R.	Cen. L	A	B
13,000					0.	0.		
26,000	.0162				0.	0.		
52,000	.0333	.0495			.02			
78,000	.0331	.0876			.04			
91,000	.0191	.1067			.04			
104,000	.0179	.1246			.04			
13,000		.0239			.02			
117,000	.1463				.04			
130,000	.0218	.1681			.04			
143,000	.0235	.1916			.06			
13,000		.0355			.04			
156,000	.2101				.06			
162,500	.0112	.2213			.06			
169,000	.0108	.2321			.05			
175,500	.0119	.2440			.06			
132,000	.0121	.2561			.06			
13,000		.0458			.04			
195,000	.2826				.06			
208,000	.0248	.3074			.02			
221,000	.0281	.3355			.02			
234,000	.0335	.3690			.02			
13,000		.0898			0.	0.		
240,500	.4028				.02			
244,400								
13,000		.2400			.16			
228,800								
O. Top flange.		.3300	.34	.430		2.40	34 L.	3.00 L.
O. Bottom flange.		.8159	.52	.715		.23	0.	.30 L.

The deflections began to increase under a stress of 36,970 lbs. per square inch on extreme fibre, and the maximum load applied was equivalent to 49,650 lbs. stress on extreme fibre. The co-efficient of elasticity deduced from the deflections within the elastic limit was 27,070,000 lbs.

Specimens cut from uninjured portions of the beam were then tested on the Olsen machine at the Union Iron Mills. A piece from web of beam gave 40,610 lbs. per square inch as the limit of elasticity, and 70,060 lbs. per square inch maximum, 27 per cent. elongation, 55.93 per cent. reduction, and 30,420,000 as co-efficient of elasticity. Piece from flange gave 40,080 lbs. per square inch as the elastic limit, 71,240 lbs. per square inch maximum load, 25 per cent. elongation, 61.23 per cent. reduction, and 29,175,000 lbs. as co-efficient of elasticity. Both of the preceding specimens being tested in tension.

Two specimens from flange of beam were also tested transversely; the first being placed on supports 14.4 inches apart, center to center, and load applied midway between bearings. In this increased deflections were noted at 60,290 lbs. stress on extreme fibre, and the maximum load sustained was equivalent to 102,490 lbs. on extreme fibre; the co-efficient of elasticity was 31,790,000 lbs.

The second specimen was placed upon the same supports, but loaded at two points in a similar manner to the test of beam. The first indications of increased deflections were observed when a load producing a strain of 53,110 lbs. in extreme fibre was applied; the maximum load was 4,850 lbs., 2,425 lbs. on each point, and was equivalent to 99,125 lbs. stress on extreme fibre; co-efficient of elasticity, 31,950,000 lbs.

Analyses of borings from these beams gave the following results:

BEAM.	Carbon.	Mang.	Phos.	Silicon.	Sulph.
Open-hearth, flange,	.20	.96	.085		
" " web,	.20	.96	.082		
Bessemer, flange at base web,	.12	1.20	.082	.035	.079

The tests of beams were made in the hydraulic machine of the Keystone Bridge Co. The deflections and elongations in all tests were measured with an electric micrometer, and are to be relied on as being extremely accurate.

In the test of the Bessemer beam, after the deflections had been taken under the load of 240,500 lbs., the operator was instructed to increase the load to 247,000 lbs., but when 244,400 lbs. was on the deflections began to increase so rapidly that there was danger of breaking the measuring apparatus, and the load was removed, the micrometer taken off, and operator told to break the beam. When the load was equal to a total of 228,800 lbs. the beam crippled, giving a good illustration of what the lay press calls a phenomenon when a bridge happens to fall under the lighter following train.

At 10 o'clock Society adjourned.

S. M. Wickersham,
Secretary.

ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

Society met at 8.10 P. M., December 20th, 1887. Present: twenty-nine members and two visitors. President Dempster in the chair, Vice President Becker assisting.

H. Ward Verner, Chas. I. Travelli, Jas. I. Simpson, S. P. Dame, Lewis C. Weldin, Hugh C. Campbell, Geo. M. Engle and Frank Felker were elected members.

The Committee on Grade Crossings made the following report, which was unanimously adopted.

To the Engineers' Society of Western Pennsylvania:

GENTLEMEN: Your Committee, appointed to inquire into the necessity of dispensing with the various railroad grade crossings on the leading thoroughfares of Pittsburgh and Allegheny, respectfully report: That they have made a cursory examination of the subject, having visited the different grade crossings and noticed the peculiarities of the several situations. Considering the recent deplorable accidents, and the likelihood of their repetition with greater frequency in the future, and considering the rapidly increasing volume of city traffic, especially on the streets affected, together with the great and growing increase of business of the railroads entering here, and crossing the streets, causing the almost continuous movement of trains at certain hours, great inconvenience and interruption to city traffic, and serious interference with business, as well as increasing the liability to accidents by such interference, your Committee find that the grade crossings, or the bulk of them, are serious and dangerous obstructions, and that they should be dispensed with.

They are of the opinion that the proper way to relieve the Fort Wayne railroad crossings on the principal thoroughfares in Allegheny, viz.: Federal, Sandusky and Anderson streets, and on the two leading thoroughfares of Pittsburgh, viz.: Penn avenue and Liberty street, is by elevating the tracks of the Fort Wayne Railroad (with its connection, the West. Penn. Railroad), so as to carry the line over the streets named, by a viaduct extending from West Park, Allegheny, to a point near Fifteenth street on the Penna. Railroad, Pittsburgh. The grades of the railroad between the points named favor such an arrangement, and the elevated road will cross the river at an elevation of about twenty feet above the present rail level, or on a level with the top chord of the present Fort Wayne Railroad bridge.

The clearance height of the railroad, at street crossings, should be at least thirteen and one-half feet.

The tracks of the Fort Wayne Road, west of the West Penn. R. R. Junction, could occupy the territory now covered by the West. Penn. Road to Federal street, and the old canal bed, west of Federal street, so far as it extends parallel with present Fort Wayne tracks.

The West. Penn. Railroad could occupy the ground now used by the Fort Wayne tracks, within the same limits.

The West. Penn. Road (west of the junction), would then lie wholly on the north side of the Fort Wayne tracks, instead of south of these tracks, as at present, and terminate in a Union station, to be located west of Federal street, or at the present Fort Wayne station.

The Fort Wayne freight yard at Penn street, Pittsburgh, to be operated, as at present, by switches from the line, running at the level of lower deck of bridge, and connecting with main line in Allegheny, east of Anderson street. This arrangement requires a double-deck bridge, or a high and low level bridge. There is territory south of the old canal-bed, along Lacock street, west of Federal street, suitable for a Fort Wayne delivery yard in Allegheny.

By such an elevated road the Fort Wayne and West. Penn. Roads will be greatly benefited by the increased facilities afforded in the running of trains, the handling of freight and passengers, and in other respects; also by being relieved of the expense of maintaining flagmen and gatekeepers, and of the liability for damages by accidents at surface crossings.

The two cities will be gainers by being relieved of the dangers and interruptions connected with grade crossings, and by the enhancement of the value of property in the vicinity of such crossings, on account of the removal of obstructions in the streets.

The length of line to be changed is about two miles. The estimated cost of this change is approximately $1,300,000, including the cost of alterations of the bridge over the Allegheny river. Even if the cost should reach the sum of one million, five hundred thousand dollars, which we think an excessive esti-

mate, it would not be an unreasonable project, considering the benefits, both direct and indirect, to be derived by the railroads and cities affected.

In addition to the crossings named, there are on the line of the Fort Wayne Road, in the western part of Allegheny, three other crossings, viz.: North avenue, Pennsylvania avenue and Washington street, of somewhat less importance at present, but which in the near future will have to be changed, if proper regard is to be given to the development of this section, and to the protection of the citizens; and to the wants of the railroad by giving it the uninterrupted use of its lines at its yards and shops located there.

The road passes over a summit in this section and will naturally be depressed; *i. e.*, will pass the streets named through a cut. This will give favorable ground at the crossings named, for over-head street bridges. Although such disposition of crossings will doubtless be made at some future time, this work is not included in the above estimate for change of road in Allegheny.

There are also crossings on the West. Penn. Road, east of the junction—such as Madison avenue, Chestnut street, and others—that need to be changed. This road will sooner or later be elevated in this vicinity, and for some distance through the city, giving underground crossings for the principal streets.

For the next important railroad crossing, viz.: Second avenue, Pittsburgh, the Committee would recommend the abandonment (for wagon traffic, at least) of this crossing, and the diversion of its travel by opening a street from a point on Second avenue, east of Try street, along the hillside, connecting with Forbes street and Fifth avenue; and by overhead bridges with Third and Fourth avenues; and further, by utilizing the undergrade crossing at First avenue, and the short street opening into Second avenue, nearly opposite the proposed new street. Fourth avenue now has a surface crossing; Third avenue at present terminates at the railroad.

The conformation of the ground at Third and Fourth avenues favors overhead street-crossings. Such an arrangement will prove of great benefit to this locality, by elevating the general building level above the bottom of the valley, and equalizing the grades of streets running from Boyd's Hill across the valley to the improved part of town, and will insure its early and rapid development.

This arrangement of bridge-crossings will also accommodate the portion of the city using Second avenue as the main artery of travel, and give additional outlets for the traffic which is now confined to this thoroughfare, already burdened with the obstructions of a grade crossing. Second avenue should eventually be provided with a foot bridge.

The bridges named will have to be placed at such an elevation as to give at least eighteen feet clear headroom above tracks.

The cost of the bridges (including foot bridge), with approaches, will approximate $50,000.

The travelling and business public will be inestimably benefited by the change. The heavy wagon traffic of Second avenue is greatly interfered with at present; and as business depends on the expeditious movement of material, it now suffers severely.

The enhanced value of property in the vicinity, by reason of such improvements, would soon equal the cost, and the railroad would be largely benefited by its increased capacity for the transaction of business at this point, and by having complete control of its roadway; also by the elimination of features calculated to make inroads upon its earnings in the way of damages for crossing accidents, and the expenses for flagmen, &c.

There are other railroad crossings in the two cities that are dangerous and need attention, such as Butler street crossing, A. V. R. R., Penn and Liberty street crossings, A. V. R. R., 18th street crossing, P., V. & C. R. R.; but those named first are the worst, and their continuance as surface crossings is fast becoming (with the increase of traffic on the streets and the increase of business on the railroads) an unbearable annoyance and positive danger to the public, and a hinderance to the proper growth and development of the two cities. Some remedy must therefore be found.

The elimination of street-crossings will benefit the cities indirectly by placing the suburbs much nearer the business centers in point of time, as the trains can then be run at a much greater speed.

It is believed that the above suggestions give a reasonable solution of the matter. Other plans may be suggested equally good, or, perhaps, preferable.

It is not the province of this Committee to go into details, or to presume to develop plans for the railroads.

Your Committee would urge that Councils of both cities be petitioned to ask the railroads to present plans for the improvement of crossings, and to act with City Councils in developing a plan to relieve the two cities of the very serious evil of surface-crossings.

Similar recommendations, in relation to Fort Wayne Railroad, were made by the City Engineer of Allegheny to City Councils in May, 1874. A plan was then made and filed with the report, and can be seen at the City Engineer's office.

Respectfully submitted,

W. L. Scaife,
Chas. Davis,
Chas. Ehlers.

Mr. Ramsey opened the discussion of his paper by reading, viz:

"I regretted my inability to be present at the last meeting, but my sorrow over my absence was somewhat mitigated by the thought that I thereby escaped the direct impact of the blows delivered by my friends Buchanan and Roberts; for at this distant point, the force is modified considerably.

Mr. Roberts assails me with "The theories and conclusions of a quarter of a century ago," and with "Experiments made as long ago as 1860;" and Mr. Buchanan, with "Hot shot" fresh from his own experiments.

Mr. Roberts always gives zest and interest to anything he says in his speeches, or may write about; even if it is something which we have all read——in the books. But it seems to me, that in referring to the experiments of Kirkaldy, Webster and other authorities, as refuting the claim of "brittleness of iron at low temperature," that he is wasting time, as I expressly admitted in the opening lines of my article, that the old authorities did not agree with me; and I also admitted that "general conviction" was opposed to such a belief. Why, if the recognized authorities and "general conviction" agreed with me, what would be the use of any experiments being made, or wasting time boring this society with an article upon a matter regarding which there is no dispute?

But, does Mr. Roberts, or any other member, who has examined the tests of Kirkaldy, Styffe, etc., say that these tests were made with direct reference to resistance to impact, or indeed (except in a few isolated cases) any reference at all to sudden strains. The Kirkaldy tests referred to, were, as Mr. Roberts well says, very inconclusive, in regard to the effect of temperature, or to anything else, other than to show the difference in ten pieces of iron, out of the same bar, but forged into ten bolts. Would any member expect exactly the same results from each of these ten pieces, even at the same temperature? Why, suppose that I had come before this society, and stated that I had taken a forty foot bar of 4 1-2 x 1 1-4 iron, and cut it into eighteen inch pieces, and then forged each piece into a bolt, and that the difference in the strength and character of the fracture was due to the temperature, would you not have run me out of the hall?

Mr. Roberts recognizes this weak point, by saying that, "he would understand that this forging spoken of had been done before the bar was cut." The report of the test does not so state.

Quoting Mr. Roberts again, "Mr. Kirkaldy's experiments were not numerous, but care seems to have been exercised to have the samples from the same bar;" true, but was much care taken to have the samples exactly alike in everything except temperature?

Again, "The experiments seem to be inconclusive, but show enough to raise a conflict with Mr. Ramsey;" it is not worth while to take your time to show the "inconclusiveness" of these tests by analyzing them. Especially as we have Mr. Buchanan's tests (which have been made expressly to show the fallacy of Mr. Ramsey's conclusions) to analyze.

Mr. Becker at the October meeting stated that Mr. Buchanan had made some tests, to see if Mr. Ramsey was right, and that he was "loaded;" I therefore awaited the explosion promised with some trepidation. But, as Mr Becker intimated, the explosion has not been very dangerous, unless it was from the recoil, like the Irishman's gun, which knocked the Irishman flat, while the rabbit escaped unharmed; the Irishman said, as the rabbit ran away, "Ye dirty spalpeen, it was a mighty fine thing for ye that I did not point the butt of the gun at yees." I am glad the gun was pointed at me, because it has "kicked"—badly—and the recoil has knocked Roberts' Kirkaldy's tests flat.

Mr. Buchanan says, "He (Mr. R.) has said nothing, however, that is particularly new." I agree with Mr. B., as he will learn if he reads the first line of the second sentence of my article. He also says that Mr. Ramsey's paper "is prepared in such a manner, that it is pretty hard to question his statements." Well, Mr. Buchanan may have found some difficulty in this matter, as the results of every one of the tests given in his table fully corroborates the statements of Mr. Ramsey, although made to show the error of his ways.

To get rid of some superfluous matters which have been tacked on to the original questions, I will state them again:

1. Does a low temperature decrease the strength of iron to *resist impact*, or make it brittle?

2. Does it increase the tensile strength, when the stress is *gradually* applied?

3. Will iron broken at a low temperature, by impact, or by nick bending tests, show a crystalline fracture, when the same iron at a higher temperature will show a good fibrous fracture?

To all of the above questions I am led to give an affirmative answer, as the result of the tests I have made.

Now, what are the results of Mr. Buchanan's tests? He does not give any tests by impact, showing the number of blows, but all the other tests made by him, without exception, answer the second and third questions in the affirmative, and give almost the same figures as mine did; as the following comparison will show.

TABLE I, SHOWING THE AVERAGE RESULTS OF TENSILE AND NICK-BENDING TESTS BY BUCHANAN AND RAMSEY.

Tests.	Nick-Bending. Low Temperature. Temperature.	Nick-Bending. Low Temperature. % Crystalline.	Nick-Bending. Low Temperature. % Fibrous.	Nick-Bending. High Temperature. Temperature.	Nick-Bending. High Temperature. % Crystalline.	Nick-Bending. High Temperature. % Fibrous.	Tensile. Low Temperature. Temp.	Tensile. Low Temperature. Elastic Limit.	Tensile. Low Temperature. Breaking Stress.	Tensile. High Temperature. Temp.	Tensile. High Temperature. Elastic Limit.	Tensile. High Temperature. Breaking Stress.	% Increase Elastic Limit by Cold.	% Increase Ultimate Strength by Cold.	% Decrease Reduction.
Ramsey.	10° 12°	25.	75.	70° 60°	0.	100.	0° 12°	31338	53130	70°	29023	50900	3.97	4 38	5.08
Buchanan	25°	26.9	73.1	80°	0.7	99.3	32°	30566	51978	80°	29550	50420	3.44	3.09	2.60

The foregoing table gives the result of about fifty tests, at each temperature (high and low), and from specimens as nearly alike in all points, excepting the temperature, as it was possible to have them. Buchanan's tests alone cover sixty specimens, and mine about eighty, and yet Mr. Roberts thinks that Kirkaldy's ten tests of forged bolts "raises a conflict with Ramsey's." Mr. Buchanan also thought that his tests would "raise a conflict" with Ramsey, but do they? Will someone tell me where there is any conflict shown in the above table? The average of Mr. Buchanan's tests by nick-bending shows that at 80 degrees it is practically all fibrous, and at 25 degrees the average is 26.9 per cent. "crystalline." Ramsey's tests show at 10 to 12 degrees 25 per cent. crystalline, and at 60 to 70 degrees 100 per cent. fibrous. Was this crystalline fraction due to too much phosphorus or impurities in the iron? Let us look at the chemical analysis.

The average results of the chemical analysis of the iron tested by both Ramsey and Buchanan were as follows:

	General Average of Tests. Phos.	Car.	Silica or Slag.	Sulp.	Man.	Averages for Warm and Cold Tests. Temp.	Phos.	Car.	Silica or Slag.	Sulp.	Man.
Buchanan.	.249	.083	.791	.018	.065	30° 80°	.250 .249	.083 .083	.821 .762	.008 .027	.073 .076
Ramsey.	.216	.10	.411	.025	.15	0° 70°	.210 .217		.520 .540	.023 .028	

These figures do not seem to show any material variation in the impurities in the iron used by Ramsey from that used by Buchanan.

Mr. Becker had made the statement, upon Mr. Buchanan's authority, that the researches made by Mr. Buchanan would show that "whenever impurities are eliminated to such an extent out of iron as to make it suitable for bridge purposes at all, then the striking variation that is shown by these tests (Ramsey's) to a large extent disappears." After such a statement we would infer that Mr. Buchanan did not think that the iron selected by Messrs. Hunt and Ramsey for the tests made was "at all suitable for bridge purposes."

This statement is not supported by the facts, as shown by his analysis; and when we compare the breaking stress, elastic limit, and reduction of area, the iron used in the Ramsey tests shows up slightly ahead of the Buchanan iron. The average reduction of area was:

Ramsey, 70°—31.31 per cent. 12°—29.73 per cent.
Buchanan, 80°—27.00 " 32°—26.35 "

The ult. stress and elastic limit is given in Table I.

Impact alone is the only fair way to test for brittleness.

I am sorry that Mr. B. did not give the ultimate stress and elastic limit for the quick breaking tests.

Mr. Buchanan's tests seem to have been carefully made, and are worth more than any amount of theorizing, as they go to show just how iron acts under certain conditions; in other words, they are direct evidence, either one way or the other, and it is only by such evidence that the matter can be settled, and I for one am glad that Mr. Buchanan has made these tests, and hope he will continue his experiments. We should join hands in such investigations as these, and only by joint investigation in which we have full confidence can this matter be definitely fixed.

Mr. Roberts says, "If the tensile strength of iron is increased by cold, there must be a rule for it." I do not know that there is or can be any regular increase in direct ratio to the decrease of the temperature, and it is rather early to try to find a rule for "cause and effect," when we do not believe in either the "cause" or "the effect;" still it may be worth while to give you the result of an examination of Buchanan and Ramsey with this object in view.

The extremes of temperatures in the tests were: Buchanan, lowest, 32°; highest, 80°; variation, 48°. Ramsey, lowest, 0° and 12°; highest, 70° variation average, 64 degrees. The percentages were as follows:

Buchanan, aggregate increase,	Tensile St. 3.09	Avg. per D.	.0644
	Elastic Lt. 3.44	" "	.071
Ramsey, "	Tensile St. 4 38	" "	.0684
	Elastic Lt. 3.97	" "	.062

It would require a large number of tests at various temperatures to show the ratio of increase.

Now, as friend Roberts has said, that it would afford him great pleasure to "catch me on a theory," I am going to avoid them as much as possible, and will therefore ask questions instead of advance a theory. Mr. Reese says there is no fibre in iron "*per se*." I would like to ask him, or some other iron expert, if iron has any fibre to it at any time, or can it be given fibre by manipulation? If the molecular theory is correct, can we obtain any fibre? Can we obtain, by rolling the iron, anything more than a slight elongation of the molecule, if even that is possible? When we speak of fibrous iron do we not mean iron in which the molecules have been so well cemented together, by proper puddling, and the use of proper material, that, when broken by gradual strains, the molecules are drawn and pulled away from each other, and by the gradual flow gives the appearance of fibre? On the other hand, is not the crystalline fracture due to the bad cementation of the molecules, or to a quick separation of the molecules, which does not give time for them to flow or move on each other?

Mr. Roberts will please understand that there are no theories embodied in the above; I am only seeking information.

Regarding the difference between "crystalline" and "granular" fractures, even the professional inspectors differ as to whether it is one or the other, and so I will not waste time over that point. Crystalline is simply a good way to designate short or brittle fractures in iron which have a crystalline appearance to the eye.

Mr. Ferris draws conclusions from a test of five car axles, broken with the thermometer down to 2 degrees below, which I think are hardly warranted by the test made. He broke five axles; three by ten blows of a drop, reversing the axle after each blow. The fall of the drop was not sufficient to break the axles, except gradually, and Mr. Ferris says that they became considerably warmer, and that when finally broken by the bending backward and forward, the fracture was fibrous. The two test pieces, which were broken by what might be called a cold test, gave a "so-called crystalline fracture to the depth of seven-eighths of an inch," or probably 20 per cent. Mr. Buchanan would have called it a crystalline fracture. None of these axles were broken at a normal temperature, so we have nothing to compare them with.

I do not claim that iron cannot be made, or that there is no iron now made which will be tough and ductile at low temperatures, and good axle iron will probably come nearer to being so than bridge iron will.

Mr. Buchanan: What made me speak of iron improving as the impurities were eliminated was the fact that in those pieces in which I got the best results there were the least impurities. When I had finished my work and sent in my report I thought I had found a point there, but afterwards I learned of some iron being made which upset that idea. It was made four times, and everything possible was done to get as good iron as the men in charge knew how to make, but in spite of all they could do it would show crystalline fractures at a low temperature.

These bars were nearly square, 2¼ by 2¼ or 2¼x2¼, some size of that character. After the fourth failure, the same stock was taken and rolled into entirely different shapes. The fractures of this iron, re-rolled into other shapes, gave the most beautiful results at any temperature. Rolled again into these large rectangular shaped bars and it was crystalline, when broken at a low temperature. That iron was almost free from impurities. The phosphorus was was .05, I think, away down. So then I began to think there was, perhaps, some other thing that entered into this matter — perhaps the manner of working the iron, the manipulation it received in

putting it into these different shapes, and then when Mr. Koch, at our last meeting, spoke of the experiments he made, in which he soaked iron in salt water and in solution of hypo-sulphide of soda, I looked over my tests and I found that the largest sizes, those sizes which had the least amount of work on them showed the greatest percentage of crystal, and as these bars I spoke of, these large rectangular sections, were somewhat of the same character, I began to think from the fact that all of these tests, both those made by Mr. Ramsey and myself, had been made by soaking the iron in a mixture of ice and salt to obtain low temperature, that perhaps we got these same conditions that Mr. Koch spoke of.

Since then I have made some more tests with special refined iron of small sections, and I find that under impact tests at low temperatures there were mere traces of this crystalline appearance, and this after leaving the iron in the mixture for an hour and forty minutes. So that I am still further convinced that the trouble is in the process of manufacture and not in the iron as iron. I intend to make, when we have some cold weather, more tests of this same iron, at reduced temperature exposing the iron out of doors. And I think I shall also act upon Mr. Koch's suggestion to paint some of the bars, and perhaps soak some, painted and unpainted, in the freezing mixture.

Mr. Ramsey: I would like to ask Mr. Buchanan if he would have accepted any of this iron which the low temperature tests showed up as crystalline, as good bridge iron under normal conditions. Iron, for instance, which at eighty degrees temperature is all fibrous, and which in the nicking and bending tests shows up high in tensile strength and reduction of area, also in elastic limit. Would it have passed your specifications?

Mr. Buchanan: Yes, sir.

Mr. Ramsey: Would it have passed when it shows ninety per cent. crystalline and yet fills all the other conditions of your specifications as to tensile strength, etc.?

Mr. Buchanan: Not if I had seen the crystalline fracture.

Mr. Ramsey: In regard to the average of your chemical tests here—they run very uniformly. Take, for instance, the two tests, one at twenty-five degrees, the other at eighty degrees. The one at the twenty-five degree test shows 55 per cent. crystalline; the carbon and manganese is the same in both. The phosphorus is 2 89 in the low test and .314 in the eighty degree test, showing an excess of phosphorus in the good iron. The silicon was .280 in the 55 per cent. crystalline and .250 in the fibrous.

There is only one test that you make here of your chemical analyses where there is an excess of impurity in the low temperature iron, while in the other cases the impurities were in excess in the high or good iron.

Of course, you may make other chemical analyses, but still you are confronted by this very question, that iron which shows up well in the shops and will fulfill every condition of the specifications shows up poorly in the field where there was no soaking in salt water, but simply frozen out of doors along with the other iron. There may be an explanation of the whole thing in Mr. Koch's remarks at the last meeting. I do not know anything about that, but if the iron be not impervious, the water penetrates and then freezes, it would undoubtedly make it more rigid, and therefore more apt to break under impact.

But the tests submitted here do not, to me, show that there were any more impurities in the crystalline iron than in the fibrous iron.

Mr. Hunt: I would like to read from Fairbain, on "Iron Manufacture." I read it because it is to the point, and because it is one of the standard works on the metallurgy of iron.

"It is also found that wrought iron, which exhibits a fibrous fracture when broken by bending, presents a widely different aspect when suddenly snapped asunder by vibration, or by a sharp blow from a shot. In the former case the fibre is elongated by bending and becomes developed in the shape of threads as fine as silk; whilst in the latter the fibres are broken short and exhibit a decidedly crystalline fracture.

"But, in fact, every description of iron is crystalline in the first instance; and these crystals, by every succeeding process of hammering, rolling, etc., become elongated and resolve themselves into fibres. There is, therefore, a wide difference in the appearance of the fracture of iron when broken by tearing and bending and when broken by impact, where time is not an element in the force producing rupture."

I quote this, and similar statements can be taken from Percy on the Metallurgy of Iron, and also from Kirkaldy's tests, to show that the leading metallurgists use the term "crystalline" to indicate the character of fracture produced by the sheared surfaces of bundles of fibres broken across in a kind of minute series of cleavage planes, so to speak, having a crystalline appearance. Turning to the dictionary, we find Worcester gives a definition of the adjective crystalline as "like crystal; bright; clear," and surely the definition is answered in the appearance of the fractures called "crystalline." Again, turning to the dictionary, we find Worcester defines the adjective *granular*, which is used by those objecting to the term *crystalline*, to describe the same appearance in wrought iron fractures as "consisting of or *resembling* grains," and grains in this connection mean minute particles of uniform appearance. I think the term *crystalline* much more satisfactory, and would use the word *granular* to de-

scribe in wrought iron fractures those having a very fine grain and with no shining particles, fractures that in tensile specimens of wrought iron accompany, usually, ductility results between those of the fibrous and crystalline fractures, giving greater percentages of elongation and reduction of area than usually occur with the crystalline fractures, but less percentages than usually occur with fibrous results. I do not mean that the bright faces in the fractures that in Mr. Ramsey's paper he refers to as "crystalline" are the plane surfaces symmetrically arranged around their axis, of crystals, which Dana, in his Mineralogy, says are produced by the laws of chemical affinity acting on the constituent molecules of a substance in its transition from a fluid to a solid state, or that the microscope or magnifying glass will not show that these bright faces may be either concave or convex instead of plane.

I do not think that Mr. Ramsey claims, and he certainly does not say in his paper, that under varying temperatures from 100 degrees Fahrenheit, to say zero degrees or below Fahrenheit, that there is a molecular charge in the form or size of the contained minute crystals of iron, such as would be indicated by considering the bright faces in the crystalline fracture of wrought iron as being the plane faces of crystals symmetrically arranged around the axis. He refers to the particular character of fracture technically called by metallurgists as crystalline as indicative of a short brittle material, or, at least, of a tensile, transverse, or shearing strain having been exerted upon it of a kind either so sudden or so severe in amount, or of the conditions of temperature, that it was unable to stand the punishment, and that it gave way short and brittle with but little blow of the metal or ductile property of the material being shown, and with the bright faces of the sheared fibres giving the pecular bright faces appearing *like* crystals.

Another point with reference to the percentage of phosphorus and silica in the analysis of the iron which had been experimented on. Good bar iron, that which gives the best results for tenacity and ductility together, speaking as we would with reference to steel, is ordinarily high in phosphorus—that is, it contains about 2-10 of one per cent. of phosphorus. When wrought iron is low in phosphorus, as it is when puddled from a pig metal down to a point in phosphorus, which, in the trade, is called "Bessemer" iron, it ordinarily is weak in tenacity.

I know that in speaking of these tests quite a number of gentlemen, especially those interested in the steel trade, have said that the iron was poor because it had high phosphorus. I would state that the per cent. of phosphorus, as well as the rest of the analyses of this material which was used by Mr. Ramsey in his tests, and also that used by Mr. Buchanan in his, is the average found of good structural iron, and contained about .2 per cent. of phosphorus.

With reference to the percentage of silica reported in the analysis of both Mr. Ramsey and, probably, Mr. Buchanan as well, it did not exist as silicon either chemically, combined or alloyed with the iron, but as oxide of silicon, *silica*, and was in the slag contained in the wrought iron as silicate of protoxide of iron. The slag in wrought iron usually contains about fifty per cent. silica, so by doubling this percentage of silica reported in the samples the amount of contained slag will be approximately known. This slag, which is mechanically mixed with the iron interspersed between the elongated bundles of crystals, is what gives the fibrous character to wrought iron.

Mr. Dempster: That is very good, but it occurs to me that the condition of this iron under different temperatures is what we would like to hear from you. The question is, as I understand it, will iron have different features or qualities when the temperature is down to zero or below as when it is 60 or 80 degrees—in other words, what is the effect of cold on it?

Mr. Hunt: I acknowledge that in my remarks I have wandered away from this point, but I simply answered the remarks of others in print. I have nothing further to say than has been said. I have made a good many tests, and they are all to the effect that iron under impact at low temperatures is more brittle than at a higher temperature.

Mr. Dempster: In other words, that the cold makes the iron brittle?

M. Hunt: That is it.

Mr. Ramsey: The question to my mind is this—is the iron brittle at low temperature or more brittle than at a normal temperature, or does it produce that appearance when broken which would cause it to be rejected at the nick-bending test, where iron must not show a crystalline fracture.

The point is right here. We rejected all of the tension members of a bridge because some of the rods broken in the field in the winter time showed 40, 50 and even 60 per cent. crystalline. All of that iron had passed at the shop, had passed at the mills and of course in the test rooms, and had been accepted. It was broken in the field accidentally—one bar. I then caused some more to be broken intentionally right where they were erecting the bridge. They showed this crystalline appearance in the fractures which in our specifications would cause the iron to be rejected.

The bridge makers were sent for. Mr. Bouscarren was called in as between the two parties, and he said that under no circumstances would he permit that iron to go in, which had been my position in the first place. The bridge builder said he was willing himself to take the iron back. They did take the bridge

back at a loss of more than $2,000. Afterwards I had them take some of the same iron down to the shop and test it in the same way and the iron proved, at a normal temperature, just what it had in the shop. There was a loss of $2,000 due to ignorance on our parts because we had gone by authorities which said that temperature did not affect iron. The question also arises, does the present method of inspection in view of these facts meet all requirements? Do they come down to the point to find out whether iron is brittle at low temperature?

Mr. Browne: I know very little about iron. I know, however, that an increase or decrease of temperature will produce a mechanical effect equal to the amount of mechanical force necessary to effect a change of length equal to that produced by the application of heat or cold. The results of Mr. Ramsey's tests shows an increased tensile strength at a decreased temperature. If, therefore, a bar of iron is tested at a temperature of 60° and a similar bar at 32°, and if the mechanical effect per square inch of cross-sectional area due to a change of 22° be added to the result obtained at a temperature of 60°, would not the same correspond very closely with the actual tests at 32°? If this be true—and it can readily be verified —then we have a simple rule to indicate the changes in tensile strength due to changes of temperature. I think, therefore, experiments should be made to prove or disprove this theory.

Mr. Ramsey: To illustrate how iron can be broken and give a crystalline fracture, when we were making our test the ring on the drop we had broke several times just by the shock of the fall. Nothing struck the ring, but when the weight ball struck the iron, after a number of blows, the ring itself would snap right off where it was attached to the weight, and it always showed at the break a granular and crystalline appearance.

Mr. Becker: On the evening on which Mr. Ramsey read his paper, the discussion wandered off somewhat from the text and spread out to the question of the effect of temperature on iron and steel rails in the track. If I recollect aright, Mr. Ramsey made the statement at that time that, according to his experience, they broke more rails during the winter months than during the summer months, and that the number of their fractures was very large.

At that time I gave it as my experience that in the first place we had no such number of fractures in proportion to the length of track, and that I did not think that the effect of the season was so marked on our lines as he indicated.

Now, "Truth is mighty and will prevail." I am very glad that the discussion has been continued so as to give me an opportunity to state that, since that time, I have looked somewhat over our records, and I am willing to make a correction so far as that particular point is concerned.

If the Society will pardon me for making a little digression, I will, in connection, show the manner in which we are keeping our rail records. We use a graphical diagram that will illustrate the manner in which we have ascertained the number of fractures on our road, on which we are keeping a general record of these failures.

We have adopted this system partly in order to answer the questions that are propounded to us by the State officials in the different States—Railroad Commissioners and Commissioners of Statistics—who ask all sorts of conundrums that the railroads must answer so the Commissioners can make their reports to the Governors. They ask such questions as these: What is the average life of a telegraph pole? How long will a cross-tie last? How long a fence post? What is the life of a rail? and so on, and we are bound to answer these questions, or stand a fine. But if the Commissioners were to ask such questions as "How large is a piece of chalk?" they would get about as much intelligence as they get in the answers to their other conundrums.

Still, in order to gain information for ourselves, I devised, some years ago, a graphical chart as a means of affording a ready reference upon which we could keep our records, and I have a couple along with me to-night.

(Mr. Becker here exhibited the charts and explained the same as follows:)

These charts are simply engraved sheets on which the vertical lines represent the distances, the scale being one-third of an inch to the mile. These vertical lines indicate the mile posts. The two charts I have here represent the line from Columbus to Indianapolis, the sheets generally covering an ordinary division of from 80 to 100 miles.

The mile posts are indicated by successive numbers, and the stations, wherever they occur, at intervals, are marked by their names. The horizontal lines represent the time, each principal division of one and one-fourth inches being one year, and these are again subdivided into twelve divisions, which represent the months. We can thus mark with accuracy the exact locality where the steel rail has been laid, as well as the time when it was laid. The charts I have here are divided to cover a period of seventeen years, which is about the time I find sufficient for two ordinary renewals of steel rails in the track. When we begin to lay a stretch of steel rail, which on this particular division was begun some time in 1878, we take the brush and mark down between the two mile posts, or between the succeeding mile posts, as the case may be,

ENGINEERS' SOCIETY OF WESTERN PENNSLYVANIA.

SLIDE MOMENT DIAGRAM,

For finding Maximum Shears and Moments in Bridges.

J. E. Greiner, C. E.

Of the different methods used for obtaining the maximum strains in a bridge, the one based on the assumption of an uniformly distributed load has a decided preference. Not only is the drudgery of calculation lessened, in fact, by using Thacher's Instrument entirely done away with, but it is argued that when the load is properly assumed, the results are such close approximations to those obtained by placing the engine, that, except in rare cases, it is likely one method will satisfy the actual conditions as well as the other.

The usual load specification for railroad bridges has been two typical consolidation engines followed by a certain weight per lineal foot, but during the last few years the heavy consolidation is accompanied by mogul and passenger types, and the calculator must find which engine will give maximum strains in each particular member.

Even to the adept, aided by a long experience, tables, slide rules and every facility for hurrying on the work, this is very laborious, while to the occasional calculator without such aids, the figuring of one simple Pratt Truss represents such an amount of labor and waste of mental energy, that *laborious* and *undesirable* very inadequately express his disapprobation.

No wonder, then, so many able enginers engaged exclusively in designing bridges, in their natural desire to simplify tasks, advocate uniform loads, and no wonder that the general engineer, who only occasionally makes such designs, cries down the wheel loads and asks wherefore such excessive refinements when there is an ample factor allowed for ignorance and uncertainty—since most of these specified engines are merely types of what exist on paper only, why not specify at once an uniform load or an uniform load with locomotive excess at head.

When some expert divulges a simple modification of an uniform load which will give identically the same strains as produced by the engine in question, then we can cast aside our moment tables and diagrams. But no uniform load alone can be assumed which will give correct strains in both web and chords. True, in some members the strains will be *about* correct, but in others there is a wide difference.

The author cannot altogether agree with the advocates of the simpler method, when they state that an uniform load which gives the same centre moment in spans up to about 65 ft. and same end shear when above this, will, with the exception of a few cases of double intersection trusses, give results so nearly identical to those produced by wheel loads, as to amount to no essential difference in the distribution of material. Of course the whole question hinges on *what* amount is considered no essential difference, the advocates of one method being very liberal, while those of the other, exacting. The line should be drawn somewhere. In our office we consider a greater variation from the wheel load than 2-10 of a square inch section, or say 2,000 lbs. strain, incorrect, and require sections to be increased when short more than this amount; and, as we have found in many cases much greater differences, we have come to the conclusion that uniform loads, except for plate girders and other special cases, are not reliable when accuracy is desirable. It stands to reason that, when an engine is placed on a particular span, say of seven panels, and the uniform load deduced, which will give the same end shear as the engine, this end shear answers only for the span in question of seven panels, and will not give correct end shear if applied to same span with either six or eight panels. Further, when the uniform load is sought the span is considered without panels and engine placed regardless of such—from this it is also evident that when span is divided into panels the case is entirely different, and results different, even for the end shear which from the assumption is supposed to be correct.

The author does not say it is impossible to find an

uniform load which will give such close results as to allow the most exacting no chance for complaint, but such loads must have an excess at head, and this excess will have to be some function of span, number of panels, and uniform load. Most railroad engineers check over the strain sheets presented by competing bridge companies, and when careful men, use the wheel load method. By this is meant placing the engine so as to give maximum shears or moments, therefore it is not absolutely essential for the bridge companies to go through with the same labor, inasmuch as the simpler method will give an estimate of *weight* and *cost* practically the same, the excess in some members balancing the scantage in others, while any difference in strain can be easily rectified. Railroad engineers prefer the wheel load method, not because it is so laborious and they have plenty of spare time, but because it is decidedly more scientific, the results being those sought, *i. e.*, the maximum strains produced by typical engines, and it is a *certain* check on the accuracy of assumed loads, used by the parties making bids.

When there are several engines to provide for, it will be found very convenient to tabulate the maximum panel weight for different lengths, also maximum centre moments for different spans, then once for all select the greatest of the different results. With the exception of very few cases it will be found that the consolidation type produces the greatest strains.

The next thing is to consider the most convenient and expeditious manner of applying the engine wheel loads. Three ways are offered.

1st—By Moment Tables, which give summation of moments and weights from wheel to wheel, the distance centre to centre of wheels, and distance of each succeeding wheel from first.

2d—By a Tracing Diagram, which is a sketch of the engine on tracing cloth, having summation of weights, moments and distances written at their proper places.

3d—By a Slide Moment Diagram, which has moments, etc., arranged in a similar manner to above tracing, but attached to the moveable part of a slide rule.

These different ways will be considered in their respective order.

To make out a table of moments for two engines coupled and followed by an uniform load, is not the long and tedious task that appears upon first glance. It is a problem; and like most problems, there is a simpler solution than that which first presents itself, and which will be clear with a moment's consideration before pitching directly in *medias res*. It will be necessary merely to indicate this process, because to the engineers present who use such tables, a discussion will be uninteresting.

The loading used in the following illustrations is that used by the B. & O. R. R. one year ago, viz., two 86 ton typical consolidation engines followed by 3000 lbs. per ft.

Moment Table for two B. & O. 86 ton Engines and 3000 lbs. per ft. for one truss single track.

I	II.	III.	IV.	V.	VI.	VII.
No. of Wheels on Bridge.	Distance from First.	Distance between Wheels.	Weight on each Wheel.	Summation of Weights.	Moments due to Summation of wts. by dist. to foll'g Wheel	Summation of Moments.
1	0		6000	6000		
2	7.5	7.5	12000	18000	45000	45000
3	12	4.5	12000	30000	81000	126000
4	16.5	4.5	12000	42000	135000	261000
5	21	4.5	12000	54000	189000	450000
6	31.5	10.5	8060	62000	567000	1017000
7	36.5	5	8000	70000	310000	1327000
8	42	5.5	8000	78000	385000	1712000
9	47	5	8000	86000	390000	2102000
10	55	8	6000	92000	688000	2790000
11	62.5	7.5	12000	104000	690000	3480000
12	67	4.5	12000	116000	468000	3948000
13	71.5	4.5	12000	128000	522000	4470000
14	76	4.5	12000	140000	576000	5046000
15	86.5	10.5	8000	148000	1470000	6516000
16	91.5	5	8000	156000	740000	7256000
17	97	5.5	8000	164000	858000	8114000
18	102	5	8000	172000	820000	8934000
19	107.5	5.5	7500	179500	946000	9880000
					9880000	check.

From the above table it will be seen that very little labor is required in tabulating the moments for any length of span, the only multiplication necesssary being the product of summation of weights shown in column V by distance to following wheel, column III, results shown in column VI. Column VII, as its title indicates, is the sum of moments found in column VI For instance, to find summation of moments about fourth wheel, add up the moments in column VI, including this wheel, and result is 261000 in column VII, or moment about fourth wheel equals moment about third, 126000 plus 135000 in column VI equals 261000 as before.

Application of Moment Tables—

W=total weight on bridge.

N=number of panels.

p=sum of weights in advance of point where shear is required.

y=distance from front end of bridge to panel point where moment is required.

L=length of span.

Then position for

Maximum shear occurs when $W - Np \geqq 0$.

" Mom. " " $\frac{Wy}{L} - p \geqq 0$.

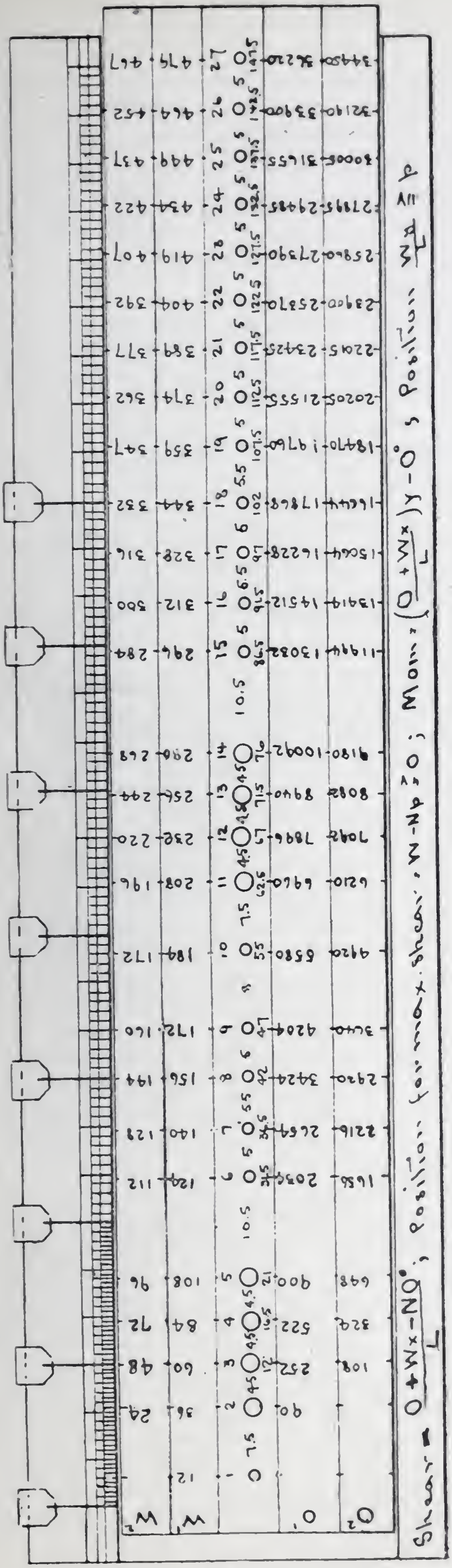


Fig a.

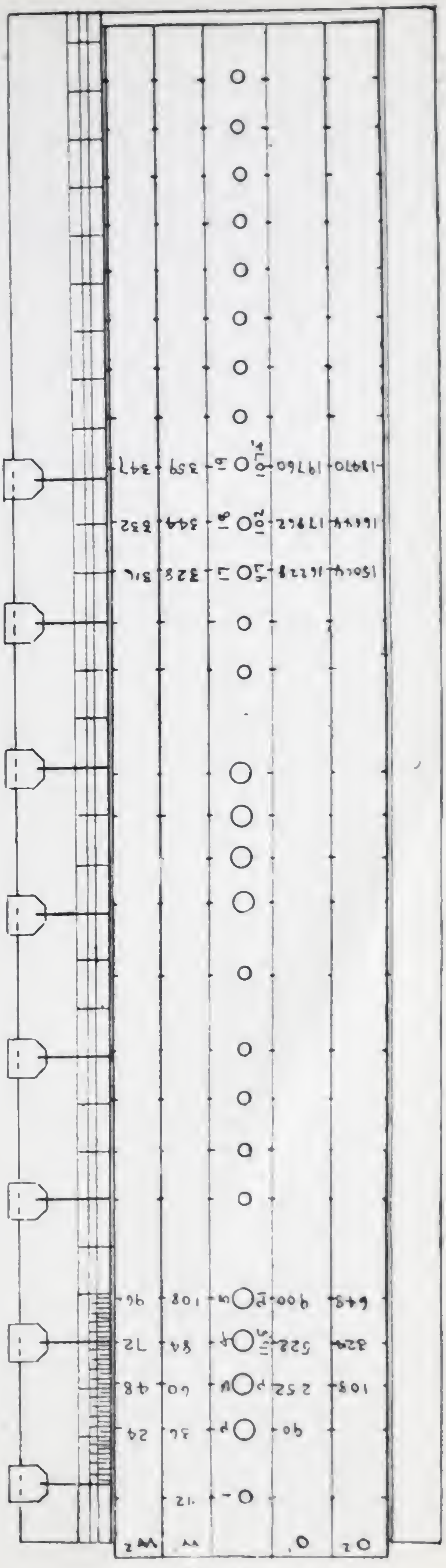


Fig. b.

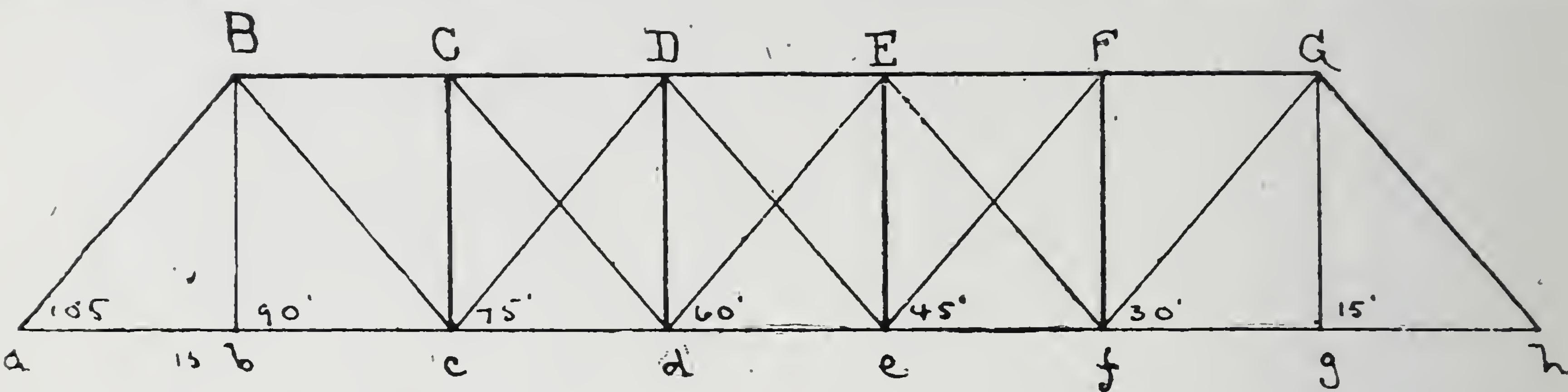
If O = sum of moment about last wheel on bridge
O° = " " " point where shear or moment is required.
X = distance from last wheel to rear end of bridge,
K = length of panel.

Then

$$\text{Shear at any point} = \frac{O+Wx}{L} - \frac{O^\circ}{K} = \frac{O+W-NO^\circ}{L} \quad (1)$$

$$\text{Moments at any point} = \frac{(O+Wx)\,y}{L} - O^\circ \quad (2)$$

The following practical example will illustrate the adaptation of table to above formulæ:



(Span L = 105′ — 0″. Number of panels N = 7, length of panels k = 15.)

Required the maximum shear in aB. Begin at either end and write down the sum of the different panel lengths, as in above figure; then proceed to find position for maximum shear, as follows:

If third wheel be placed at b, the distance from head of engine to rear end h of bridge will be 90 + 12 = 102 feet. Look along column II for the next smaller distance, which is 97, corresponding to seventeenth wheel. Opposite seventeenth wheel, in column V, find W = 164000, and opposite second wheel, in same column, p = 18000; hence,

$$W - Np = 164000 - 7 \times 18000 = +38000$$

If fourth wheel be placed at b the first will leave the bridge. Distance from head of train to end of span = 90+16.5 ft.=106.5 ft. From column II find that 102 feet is the distance from head to last wheel on bridge (the 18th). Opposite 18 find column V, W = 172000, but since first wheel is off, W = 172000 — 6000 = 166000. In like manner weight in column V, opposite third wheel, must be diminished by 6000, and we have p = 30000 — 6000 = 24000; hence,

$$W - Np = 166000 - 7 \times 24000 = -2000$$

From the conditions for position of engine for maximum shear the value W — Np must never be negative; therefore, if no wheel makes this quantity equal to zero, the nearest positive result determines the position. Hence, third wheel at "b" will give the maximum end shear on the bridge.

Having found the position, proceed at once to find shear by applying formula (1). With third wheel at "b" the distance from head of train to end of span was found to be 102 feet, and that the seventeenth wheel was last on the bridge; hence,

In column VII, opposite 17, O = 8114000
" V, " 17, W = 164000
" VII, " 3, O° = 126000
X = 102 — 97 = 5

Therefore,

$$\frac{O+Wx-NO^\circ}{L} = \frac{8114000 + 16400 \times 5 - 7 \times 126000}{105}$$

=76686, which is the maximum shear in end post aB.

It will be noticed in this particular case the summation of moments at end of span could have been taken directly opposite the 19th wheel, which is over the end of span, thereby saving the labor of obtaining the value of Wx; but in order to make this example more general, no attention was paid to the fact.

After what has preceded, the formula for moments requires no explanation, since the application is similar. It might be well to observe, however, that "y" will always be one or more panel lengths, and, therefore $\frac{y}{L}$ (in cases where panels are of equal lengths) can be simplified by making L = N and y = the number of the point in question from head of span.

The Tracing Diagram, as mentioned before, is imply a scale sketch of the engine wheels made on tracing cloth, having the distances, weights and moments, as shown in columns I, II, III, V, VII, written at their proper places over wheels. It possesses a slight advantage over the *tabulated* moments for obvious reasons, but is awkward to handle, and requires for its advantageous use a sketch of the truss drawn to same scale as diagram, so that by placing the latter on the truss the different positions can be seen at once. It lessens the chance of error in taking out the weights and moments; also often enables the expert to judge accurately in regard to proper positions without the necessary preliminary calculations given in description of moment tables. The necessity of drawing the truss to scale, besides having two extra loose sheets on desk in addition to the papers, specifications, notes and other loose arlicles required while estimating, render this appliance objectionable, and engineers would rather

refer to moment tables than hunt up this tracing and corresponding sketch, which will be often mislaid or covered up.

THE SLIDE DIAGRAM.—It was for the purpose of avoiding scale drawings, besides having the moments, etc., in such shape as not to be easily mislaid, that the advisability of attaching a tracing diagram to the movable part of a slide rule was suggested and first put into practice in the consulting engineer's office of the B. & O. R. R.

The stationary part of the rule is divided into feet with as many smaller divisions as convenient, and is provided with a number of adjustable clamps for marking off the panel points. These clamps are made of brass, having a thin steel pointer attached. They are easily moved to any distance and then made fast by thumb-screws. The slide part represents the engine with all its distances, moments, etc., as explained for the moment table, but for convenience having two additional columns, one for sum of weights, the other the sum of moments, from second wheel. The drawing shows this diagram full size up to 150 feet span. Column W^1 and W^2 are summation of weights from first and second wheel respectively. Columns O^1 and O^2 represent the summation of moments. O^2 and W^2 are to be used only in such cases where the maximum shears and moments result when first wheel is off the bridge. These two columns lessen labor somewhat, since, if omitted, subtraction will have to be made, as shown for moment tables with fourth wheel at *b*. Weights and moments are expressed in 1000 lbs., the last three figures being omitted, and are given for total engine or for one truss double track.

The rule is made of well seasoned hard wood. The diagram and scale being first made on tracing cloth are then blue printed and the blue print fastened to the wood by means of varnish. The scale should not be separated from diagram until the varnish becomes thoroughly dry, or there is danger of the two shrinking unequally, and consequently there would be uncertainty in using the scale.

The use and convenience of this appliance will be illustrated by same example as before. Adjust clamps so as to represent panels of fifteen feet, then move slide until third wheel is at *b* (see Fig. a). This position shows seventeenth wheel the last on bridge, and there is seen directly that $W^1 = 164000$, the total weight on bridge. *p*, the weight over second wheel, is 18000, hence, as before—

$$W - Np = 164000 - 7 \times 18000 = +38000^*$$

which gives the position for maximum shear in end post *aB*.

If fourth wheel be placed at *b* (see Fig. b), the first will leave the span, hence the value of $W - Np$ taken from column W^2 is,

$$166000 - 24000 \times 7 = -2000.$$

Place the third wheel at *b* and proceed to find shear by applying formula (1). The distance $X = 5$ feet, as well as all the other values of the formula, are read off directly. Hence—

$$\frac{O^1 + W^1x - NO^2}{l} =$$

$$\frac{814000 + 164000 \times 5 - 7 \times 126000}{105} = 76686,$$

which is the maximum shear in end post *aB*.

By comparing the direct way of this method with the roundabout moment tables or clumsy tracing, its superiority becomes at once apparent, so that little need be said in order to unfold its many advantages. The position for maximum moment, and the moments themselves, can be obtained just as easily as the foregoing shears, therefore any further applications in the present paper would be superfluous.

The points in favor of this method can be briefly enumerated as follows:

1. Simplicity of application.
2. Convenient size, taking up small space on desk and always ready for use.
3. Requires no preliminary drawing, but shows at once position of train on bridge, thereby reducing mental labor to a minimum.
4. The engine remains in position until moved.
5. Lessens chances of error.

In conclusion, it might be mentioned that the "Slide Diagram Method of Moments" is the most practicable manner of figuring strains when accuracy is required, and the engineer after once using such an appliance as here described will no more willingly return to tables and tracings, than he would again prefer the exercise of multiplication and division after having once become familiar with Mr. Thacher's valuable calculating instrument.

* NOTE.—These results are for single track, or ½ of weights and moments given on figures "a" and "b."